ROYAL COMMISSION ON MATTERS OF HEALTH AND SAFETY

ARISING FROM THE USE OF ASBESTOS IN ONTARIO

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Volume XXV B

ROYAL COMMISSION ON MATTERS OF HEALTH AND SAFETY

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VOLUME XXV B

INDEX OF WITNESSES:

REPORTER'S NOTE: See VOLUME XXV B for first day's evidence

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THE FURTHER PROCEEDINGS OF THIS INQUIRY RESUMED PURSUANT TO ADJOURNMENT

APPEARANCES AS HERETOFORE NOTED

DR. DUPRE: Good morning, counsel.

MR. LASKIN: Good morning, Mr. Chairman.

DR. DUPRE: Before we go over to the questions, I wanted to say to all concerned that the Commission yesterday reflected, given M. Casgrain's reminder of the invitation of the QAMA, as to when it would be most likely that we would be able to exercise our option to take it up. It's likely to be some time from now, probably in December or January, and the seasonal side notwithstanding, this is simply a reminder that for the Commissioners, in any event, (REPORTER'S NOTE: French phrase spoken here.)

Very well, counsel, do you wish to...

MR. LASKIN: I actually have a few areas, and I won't be too long, but perhaps a little longer than I originally estimated last night.

I'll do my best.

JULIAN PETO, PREVIOUSLY AFFIRMED, RESUMES THE WITNESS STAND

EXAMINATION-IN-CHIEF BY MR. LASKIN, CONTINUED

Q. I think we ended up last night, Mr. Peto, discussing the Finkelstein study, and in the light of overnight's reflection I wondered whether you had any other comments to offer us with respect to that study?

A. Not really. I mean, as I said yesterday, and I think Dr. Finkelstein said, the conversion from early measurements to modern measurements is a dubious procedure, and there were few measurements during the early period and it may be that the dust levels were considerably higher than the estimates, which would obviously affect the estimates of risk.

Q. Can I ask you specifically, in terms of the mesotheliomas, because there appear to be a fairly large number in this particular cohort, which particular dust estimates are more important or not more important than others?

Is it the earlier exposures that are...?

A. Oh, yes. I mean, that's the difficulty that in fact...because of the long interval from exposure to mesothelioma incidence, I assume that...it's difficult to answer these questions and I really ought...I wonder if you would like to ask Dr. Finkelstein.

I assume that most of the mesotheliomas arose in people who were first exposed during the early period from 1948 to about 1955.

Would that be...?

The difficulty is it's that period when the dust data are least reliable.

Q. What about the fact that there may have been, on the evidence there was some crocidolite at this particular operation? Does that have any effect?

For example, in terms of what your dust estimates may be?

A. Well, I'm not sure whether the membrane filter samples that were taken in 1969 and 1970 and 1971, and during that period which the estimates are based on extrapolation back from those measurements, I'm not sure whether at that time there was still crocidolite being used in substantial quantities.

Q. What affect might that have?

A. I don't know at all, because in fact as far as I know there aren't any decent fiber counts in areas where crocidolite was being used. I'm not aware of any that have ever been published.

So whether or not crocidolite...it's widely believed that similar processes using crocidolite produce very much higher fiber counts, so although it may be true that crocidolite is no more dangerous fiber-per-fiber than chrysotile, which produces fibers of comparable dimension, I think it's likely that the similar process using crocidolite is more dangerous. So from a legislative point of view it may be reasonable not to make as strong a distinction as has been made in England, but at the same time from a technical point of view it may be preferable not to use crocidolite.

But as I say, these are speculative suggestions and as far as I know there aren't any good data to back them up. There seems to be a much higher incidence of mesothelioma, for example, in gas mask workers who worked with crocidolite, than those who worked with chrysotile. But at the same time, people who developed mesothelioma having been exposed to crocidolite often have very high lung burdens many years afterwards, which suggests that the actual...although the exposures is not very prolonged, the actual fiber counts are very, very high.

I think it's possible that Dr. Finkelstein's estimates of fiber counts are underestimates for that reason, because there may have been very much higher crocidolite fiber levels during the early period than his estimates would suggest.

A. (cont'd.) But as I've said several times, this is very speculative. It's very difficult to...he emphasizes in his paper that these conversions are difficult and there could be quite large...they could be out by quite a large factor. This is true of most studies.

Q. The one difference that I suppose I noted as between the two studies when one looks at results as between his study and your study at Rochdale, is that while the relative risk or the relative risk figures for lung cancer are roughly comparable, seven and a half for eight and five, you have no mesotheliomas. He has apparently quite a number, and he is looking at employees who started working at roughly the same time as, I suppose, your cohort.

I just wonder whether you had any comment on that observation?

A. It's difficult to answer without looking at the distribution of age at first exposure and years and successive periods since first exposure, and so on, and doing a formal comparison.

As I say, there is in fact at least one mesothelioma now in that cohort, although it isn't within the followup period that has been described in the published papers.

Also, sampling fluctuation is quite a major factor in all these things. As I said, there were a total of eight lung cancers in the data that I presented, and the numbers in Dr. Finkelstein's study are larger than that, but not enormous.

The inconsistency may not be as gross as it appears.

Also, there is the relative risk for lung cancer, and yes, it is, compared with the figures that I presented, slightly exaggerated in his study because they were based on all cases which...in fact the data are presented in two ways. The data are presented both for cases which are certified on the death

A. (cont'd.) certificate and cases which they were aware of, and it's characteristic that there are quite a number of lung cancers in asbestos workers where lung cancer isn't the major cause of death. It's not the cause of death which is officially recorded.

It's a matter of debate how you should deal with that. I have not included them in the anlaysis that I have done, and I think that his data, in fact, they are included both ways. The highest relative risks for lung cancer I think are based on analyses which include the cases which weren't actually the certified cause of death.

Is that correct?

DR. FINKELSTEIN: Actually, the work in my table one for twenty to twenty-five years...

THE WITNESS: Yes, that's restricted.

DR. FINKELSTEIN: ...death certificates, and your twenty to twenty-five year finding of relative risk in your cohort of five point three and mine is six. You told us yesterday that the cumulative exposures were a hundred for these men, and a hundred and six for my men.

So it seems to me the results are identical.

THE WITNESS: Yes, so in fact in that case it would appear that the mesothelioma is rather high in his cohort, but as I say, I would be interested to see the data broken down, and the data aren't presented in man years in successive periods since first exposure, and to inaugurate a period as long as twenty to thirty-three years isn't satisfactory from the point of view of analyzing mesothelioma incidence because it's going up as the third or fourth power of time. There's quite a big difference between twenty to the power of three and a half, and thirty-three to the power of three and a half.

So to decide how inconsistent the results are, you would need to see the results broken down in those finer

A. (cont'd.) divisions.

MR. LASKIN: Q. Between twenty and thirty-three years?

THE WITNESS: A. Well, all the way from fifteen onwards, in five year periods, yes. With nine years in each period.

I'm sure that Dr. Finkelstein has those data and could give them to you.

Q. Your Rochdale data were based on death certificates then?

A. Yes. The difference isn't large.

Q. Did you do any best evidence calculations, or did you back behind the death certificates in any of the cases?

A. Yes. I can't remember, in fact, in that cohort, as distinct from the earlier cohort, what the numbers are, but the difference isn't large. I mean you can see...I mean I think that Dr. Finkelstein's results are typical. I forget what the difference in the number of lung cancers is, but it isn't large.

The difference in the number of mesotheliomas is quite large, but in fact for mesotheliomas we have always included all the cases that we are aware of, so we haven't in fact relied on death certificate information alone. We have used information provided by the factory, and occasionally provided by other sources, to identify mesotheliomas.

Q. At Rochdale?

A. Yes. You may know that there is a difficulty with the coding of mesothelioma. They were completely ambiguous. The International Classification of Diseases has dealt with it in a very unsatisfactory way. It can be coded under a variety of different rubrics, and it sometimes comes out as cancer of the pleura. But cancer of the pleura often is a miscoding that

A. (cont'd.) is used for cancer of the lung that appears for some...you know, whoever it is who is classifying the death as being cancer of the pleura, as far as I know, cancers of the pleura would be predominantly mesothelioma. Certainly the coding one sixty-three for cancer of the pleura isn't used exclusively for mesothelioma. Two twenty-eight is the classification for benign diseases, and the majority of mesotheliomas are classified to two twenty-eight.

I think if they don't put the word malignant mesothelioma on the death certificate...all mesotheliomas are extremely malignant...but if the word malignant happens not to be on the death certificate, that affects the coding.

So it isn't a disease that can usefully be analyzed simply by looking at the official ICD coding.

Q. How does that in turn affect the way a researcher might treat it? Assuming that most...

A. It's usually clear from what's actually written, the actual description of the tumor, which ones should be investigated further.

Q. So what you should really...what we should be looking at if we see mesotheliomas in the study, is looking to see whether the researcher has looked at codings under two-two-eight. Is that...?

A. Well, you should look at what is written under the cause of death and see whether the word mesothelioma appears. That's the main thing.

There is a further difficulty with peritoneal mesothelioma, which are grossly underdiagnosed, of course. I mean, this is something that I know nothing about because it hasn't in fact arisen. I mean, they don't occur in chrysotile workers, but Professor Selikoff's estimates, which I am sure are well founded, of the number of mesotheliomas, particularly peritoneal mesotheliomas, are very much larger than the numbers

A. (cont'd.) that are coded. He has also discovered that certain cancers, particularly I think pancreatic cancer, is very often in fact peritoneal mesothelioma which has been misdiagnosed. And some of the apparent excesses of other cancers in his studies reduce considerably when the data are re-examined and these excesses are partly, or in some cases largely, accounted for by mesotheliomas.

DR. DUPRE: I just wanted to make sure I had a couple of comparison points straight. If we are comparing Dr. Finkelstein's mortality study with your recent Rochdale study, in one instance the year of first employment is 1948, and in yours it is 1951. Correct?

THE WITNESS: Yes.

DR. DUPRE: He looked at people who were employed nine or more years, you have looked at ten or more years.

THE WITNESS: Yes.

DR. DUPRE: Right?

THE WITNESS: Yes.

DR. DUPRE: In the updated Rochdale study, the plant is still using, the Rochdale plant is still using a mixture of chrysotile and crocidolite, is it?

THE WITNESS: No, certainly not. I mean, there hasn't been any crocidolite there for quite a long time.

DR. DUPRE: There was no crocidolite in use in Rochdale from 1951 on?

THE WITNESS: Ah, well, no. According to a footnote in the Simpson Report, there was. But the evidence is anecdotal and I've really got nothing to add to what is in there. There is a footnote, there is an anecdotal report.

MR. LASKIN: This is the communication on page 46...

THE WITNESS: Dr. Gilson said he saw that several

THE WITNESS: (cont'd.) of the cards were using ...in fact I think he says a third of the cards are using crocidolite as late as 1963, which is certainly not consistent with what we were told by the people in the factory. So there the results are difficult to interpret.

DR. DUPRE: Just one other point. Your 1951 study, at this juncture reports no mesotheliomas, correct?

THE WITNESS: Yes.

One other difference: I don't know what the pattern of recruitment was, but since the plant that Dr. Finkelstein was studying opened in 1948, I assume that a large number of people were recruited at that point in time. That in fact a lot of people were recruited abruptly in 1948.

The Rochdale factory was continuing, and the only reason that 1951 was a significant point to start the analysis was that that was when routine measurements were instituted.

So the actual numbers of people recruited were very small...the numbers of people who were recruited from 1951 to 1955 were actually quite small, so he would in fact, in his study, have a much larger number of people who had been followed up beyond thirty years than we would have. In fact, I think we have virtually none followed up for thirty years.

Oh, wait a minute. I mean we analyzed from 1951 onwards in the last update, which was the end of 1978, so we have none at all. I mean, his factory started three years earlier, the followup is two years longer, and most important of all, he had a large intake at the beginning of the study, so he has substantially more data than we do during the period from thirty to thirty-five years, and from twenty-five to thirty. That makes a very big difference from the point of view of analyzing the mesothelioma incidence, and that's why it's important to compare the numbers of man years in successive

A. (cont'd.) periods before you can say whether the studies are in fact compatable.

DR. DUPRE: Do you remember offhand, what was the total size of the 1951 study cohort?

MR. LASKIN: Six seventy-nine, I think.

THE WITNESS: No, no. That was including the 1933 cohort. It was a very small group. It was of the order of...it's between two and three hundred, I think.

MR. LASKIN: Oh, yes. Two fifty-five.

DR. DUPRE: Two fifty-five.

THE WITNESS: Yes.

DR. DUPRE: Of that group, what you are telling me is that of that group of two hundred and fifty-five, only a fraction, and quite possibly a relatively small fraction, entered employment in 1951?

THE WITNESS: A very small number would have entered in 1951.

DR. DUPRE: Although...

THE WITNESS: In fact, I doubt if there were more than about fifty who entered between 1951 and 1955. The comparison that we presented of estimated exposure levels, I think included forty-two controls and the eight lung cancer cases, and they were all the people that we had exposure data on who entered between 1951 and 1955. That was the great majority, if not all, because as I say, the BOHS had already tried to collect exposure estimates for the whole cohort.

DR. DUPRE: Could you tell me ...did you just tell me that nearly all of your cohort of two fifty-five entered employment between 1951 and 1955?

THE WITNESS: No, a very small proportion of them.

DR. DUPRE: Oh, a small proportion.

THE WITNESS: I would think of the order of fifty or sixty.

DR. DUPRE: Now, given your cohort requirement of ten years employment, I would take it then that your two hundred and fifty-five entered employment probably in small numbers each year from between, what, about 1951 and 1960?

THE WITNESS: Yes, that's right.

DR. DUPRE : Because anyone who entered later than sixty would have been ineligible for inclusion in your cohort? Because your cohort required ten years of employment?

THE WITNESS: Almost, yes. I think it was ten years employment up to, I think it was the end of 1972. So in fact you had to enter in 1962 or earlier to be eligible for the cohort.

One point in relation to the mesothelioma rate, I think in the Lyon paper which is...I'm not sure which of the papers it is, but the paper on the lung cancer rates in relation to measured dust levels, in the Lyon proceedings, I worked out that the expected number of mesotheliomas, given the weight in the earlier group, would have been nought point four.

DR. DUPRE: That's what I...

THE WITNESS: If Dr. Finkelstein has observed eleven cases, and we expected nought point four, that would imply that if he actually had a comparable group, a comparable exposure, in each successive period, his rate would had to have been thirty times higher than ours, and it would certainly be a world record for mesothelioma incidence. So I doubt if that's the case.

As I say, I think the difference is that he has considerably longer observation and larger numbers followed beyond twenty-five years. I think that's the major explanation for the difference between the two studies.

DR. DUPRE: Could you just do this slowly?

The paper you refer to is tab seven in our records, if you could just bring the appropriate page to our attention.

THE WITNESS: Yes. In cohort one, which was...in the

THE WITNESS: (contd.) nomenclature of this paper...this is in tab seven...

DR. DUPRE: What page?

THE WITNESS: On page 831, the top of page 831.

There is an analysis of two cohorts, people who started work between 1933 and 1950, and people who started work in 1951 or later. I mentioned on that page that if in fact the incidence rates of asbestosis and mesothelioma that were observed in cohort one in successive five year periods since first exposure were applied to the corresponding man years of observation in cohort two, then the expected numbers of mesotheliomas in cohort two would have been nought point four. So the fact that there hadn't been any was unremarkable.

This emphasizes the point that I am making, that in fact there were virtually no man years of observation beyond twenty-five years, which is the period when the majority of mesotheliomas occur, and I think Dr. Finkelstein has very substantially more observation than we do over that particular period.

MR. LASKIN: Q. Do we see that from table one at page 830, your calculation of man years?

THE WITNESS: A. Oh, yes. There are ninety-six man years of observation beyond twenty-five years after first exposure, in this study. That's shown on table one on page 830 of tab seven.

As I say, that's why it is so important that Dr. Finkelstein's data should be broken down into five year periods from fifteen to thirty-three, because it does make a very big difference to the pattern of mesotheliomas that we expect to see.

Q. With the man years calculation?

A. Yes. In the paper which has been described as tab eleven, the paper on trends in mesothelioma incidence in the

A. (cont'd.) U.S., the expected pattern of mesothelioma is, in a particular cohort, is shown in table one. This is the pattern of mesothelioma incidence that you would expect to see in equal cohorts of men first exposed at different ages, allowing for mortality...natural mortality. This is numbers up to age eighty.

For example, in men first exposed at age eighteen and a half, which is the first column of the table, if you had a cohort in which you expected to see a total of in fact forty-nine, the number at the bottom of the column, then by age forty-five, for example...in other words, during the first twenty-six and a half years of followup in that cohort...you would have expected to see a total of approximately two cases, so you would only have seen four percent of the cases that would eventually occur with more than twenty-five years followup.

That first column in that table is more or less analogous to the followup pattern in our 1951 cohort.

Q. Where do you get the two cases?

A. Just adding up the first four numbers in that column, point zero four, plus point one seven, plus point four eight, plus one point zero seven, adds up to two. It adds up to less than two, in fact. It adds up to about one point seven.

That's one point seven out of a lifelong total of forty-nine, so you would expect to have seen something of the order of three or four percent of the total number of mesotheliomas that would eventually occur in such a cohort, with more than twenty-five years of followup.

Q. Can I show you one of the documents that Dr. Finkelstein did produce to us during his testimony, which, as I understand it, does show this incidence of mesothelioma in five year periods, although I may not have interpreted the document correctly?

That's the graph on log log paper that is

Q. (cont'd.) headed Incidence of Mesothelioma Versus Time Since First Exposure.

A. I'm sorry, it's difficult to work out what the vertical scale is. What is the difference in the absolute incidence rates? What's the ratio of absolute incidence rates at a given point in time?

DR. FINKELSTEIN: The absolute numbers can be taken from your Lyon paper, I guess, Newhouse and Berry...

(REPORTER'S NOTE: Dr. Finkelstein's comments became unintelligible here.)

THE WITNESS: But what, for example, is the absolute observed incidence twenty-five to thirty years after first exposure, in your cohort? What is the absolute annual incidence? Because it would be of the order of between one and three per thousand per annum in our data or in Newhouse and Berry's.

DR. FINKELSTEIN: I don't have the data with me. but they are all on the same scale. So you just multiply whatever the incidence is by...

MR. McNAMEE: Excuse me. Would you please come up? The reporter is having trouble.

DR. DUPRE: Why don't you sit next to Mr. McNamee, Dr. Finkelstein.

THE WITNESS: The incidence appears to be five times as high. Is that correct? Is the incidence...the vertical distance between the lines seems to be a factor of five. Is that correct? That the incidence is actually five times as high?

DR. FINKELSTEIN: Yes, that's correct.

I interpreted part of that variance as being because there is no dilution by short-term employees, which there is in Selikoff's cohort, and probably Newhouse and Berry as well. When I look at all the employees, I expect the absolute incidence rates to drop.

THE WITNESS: Yes, but the same would be true of

THE WITNESS: (cont'd.) the excess of lung cancer, wouldn't it?

DR. FINKELSTEIN: No, because the lung cancers are dose-dependencies, purely, and incidence is without relation to dose...versus time rather than dose. So there would be more man years of observation in each five year interval, but the number of mesotheliomas will not increase proportionally, so the absolute incidence will then drop.

THE WITNESS: Yes, but the relative risk for lung cancer would drop as well, if you include it, because you are then basing your expected number for lung cancer on the increased observation as well, so the ratio of lung cancer to mesothelioma isn't grossly dependent on duration of exposure.

In fact, if anything, I think the reverse is true, that the brief exposure would tend to be slightly more dangerous from the point of view of causing mesothelioma than lung cancer.

DR. FINKELSTEIN: I...really, I think when one looks at the whole cohort, the relative risk for lung cancer will drop. I think here the appropriate comparison is with your group, which essentially have the same criteria for entering...in terms of lung cancer...who have the same average cumulative exposure, which you said yesterday was a hundred fiber years.

THE WITNESS: Yes. I must say, as I said, the remarkable difference within our study was between the relative risk for lung cancer in the post-1951 employees, and the early employees. It is surprising, and I am inclined to think that our results were inflated by chance. As I said in the paper, I thought that this wasn't in fact a sensible estimate, and it was more reasonable to assume that the true relative risk was of the order of two or three. I said that in tab seven, the one that we are referring to, in the discussion of that paper. I pointed out that there was actually a statistically significant difference

THE WITNESS: (cont'd.) in the relative risks over the two periods, and I thought that these results had been inflated by chance.

So the fact that Dr. Finkelstein has seen a relative risk of the order of ten for lung cancer, and five times as high an incidence of mesothelioma, as has been seen in the Rochdale cohort, would be precisely consistent with the assumption that my interpretation of our data was correct, and in fact the true relative risk in this cohort is of the order of two or three, which would then be in line with the ratio of excess lung cancer to mesothelioma.

It would then be consistent, having a relative risk of two or three, and this mesothelioma incidence. And in his study, a relative risk of the order of ten for lung cancer, and a very much higher, a four or five times higher incidence, of mesothelioma.

As I should also emphasize, as I pointed out yesterday, that in fact the actual structure of the cohort, the length that they are followed up for, the proportion of smokers in them, and in particular how old they are when they are first exposed, makes a very big difference to the ratio of lung cancer to mesothelioma, and so it's very difficult when you are presented with, you know, somebody else's data...which obviously aren't presented in sufficient detail for you to be able to examine all these issues.

The assertion that the ratio of excess lung cancer to mesothelioma is more or less the same is a crude approximation, which is, as I say, evidently contradicted by the data that I showed yesterday from Professor Selikoff's study which show that according to age at first exposure and smoking habit, you get very large changes in the ratio. You don't, in fact, expect to have consistency between different studies.

THE WITNESS: (cont'd.) I'm not sure that we can usefully pursue this much further without actually having a look at it in more detail and knowing exactly what the distribution of man years and ages at first exposure, and so on, are.

I mean, it isn't clear to me that there is a gross anomaly between these studies.

DR. UFFEN: This won't be resolved until there has been more observation, a few more years go by. And then will it be possible to resolve?

THE WITNESS: Well, you can answer the question at the moment, whether they are actually statistically inconsistent, and whether they are consistent with each other or with a pattern that has been observed when you look at other studies.

But it's not an easy question to answer when you are sitting behind a microphone and you are seeing graphs and log log scales for the first time.

MR. LASKIN: I'm sorry about that.

DR. UFFEN: Counsel, could I put another word in?

MR. LASKIN: Yes, sure.

DR. UFFEN: Is it a difference that could be resolved or explained in due course by increased observation of cases, or is it something dependent on those crucial assumptions of eighteen years ago which we will never be able to establish?

THE WITNESS: I'm not quite sure what the point of the question is, because there are two issues. There is whether the actual...there is the question of whether crocidolite is disproportionately dangerous in causing mesothelioma rather than lung cancer, and there is the question of how these studies should be used in relation to estimates of exposure and the particular excesses that were observed in them.

It seems to me that since the absolute risks in

THE WITNESS: (cont'd.) this study are..appear to be very much higher than they are in the Rochdale cohort, certainly taken overall and even, certainly, for mesothelioma and probably for lung cancer in the later part of the Rochdale cohort, I would suspect that the estimates of the..the early estimates of dust level in this study are too low, because they have produced estimates which are really quite inconsistent with any other estimates that have previously been produced.

MR. LASKIN: Q. You are talking about other plants operating around the same time? Is that what you mean?

THE WITNESS: A. Well, no. I suppose I am talking about the Rochdale estimates and the very dubious estimates in relation to American insulation workers and Dr. Enterline's studies.

I mean, they can all be criticized, as they have been, on the grounds that the estimates are guesstimates.

I mean, we also know, or at least strongly suspect, that the fiber dimension is just as important as fiber number, and there are no data in any of these cohorts which address that question satisfactorily.

In some ways it's surprising that the range of estimates only varies tenfold between these different studies.

Q. Do I also take it, consistent with your analysis yesterday, to look at this ratio between excesses of lung cancer and mesothelioma, that the type of mesotheliomas we should be looking at is pleural mesothelioma, for the kind of analysis you presented yesterday?

A. It depends what sort of asbestos you want to legislate for. I mean, the chrysotile has apparently never caused peritoneal mesothelioma. Amosite clearly causes it...at least as frequently as pleural mesothelioma...and the data on crocidolite are quite ambiguous because there were crocidolite miners in Australia where there have been, anecdotally, fifty

A. (cont'd.) cases, and in..formally analyzed in the literature, twenty-six in the proceedings at the Lyon meeting, none of which are peritoneal.

But at the same time, the study of crocidolite gas mask workers, which I think is a Canadian study, which was carried out by Professor McDonald...it's referred to in the paper...I think it's the first paper in volume 330 of the 1979 New York Academy of Sciences...there were nine mesotheliomas, six of which were peritoneal, in a cohort supposedly exposed only to crocidolite.

So whether crocidolite causes both or not seems unclear. I mean, if it does cause both, I would assume that there must be very big differences in the fiber dimensions of different types of crocidolite, and some are physically more like amosite. That would be the obvious explanation.

It's difficult to believe that there are another fifty peritoneal mesotheliomas in the Australian crocidolite miners cohort, none of which have been detected.

Q. Thanks, Mr. Peto.

There are just one or two points I wanted to clear up from your analysis yesterday, which is still on the board. One of the things I wanted to clear up was the lifelong risk estimate of point six two five percent that appeared, that you developed from what was in the Simpson Report, and the figure that appears in your tab number three, your hygiene standard paper, at page 487, where you there suggest a lifelong risk of lung cancer attributable to asbestos of about three percent in smokers.

Are you with me...at the top of the second column on page 487.

A. Yes.

Q. Okay. Now, how does that point six two five percent figure relate to the three percent figure that's in your tab number three?

A. The relative risk, both in that calculation on page 487 and in the one that is discussed on page 76 of volume one of the Simpson Report, corresponds to...is the relative risk that's predicted to occur for lung cancer after fifty years exposure at one fiber per c.c., using the Rochdale data at face value with no adjustment for measured dust levels.

Q. And no adjustment for four hour, as opposed to an eight hour day?

A. Yes. Yes. It's just a straightforward estimate. You estimate that there was a relative risk of two at two hundred fiber years, therefore there would be a relative risk of one point two five at fifty fiber years, and that's the figure...in the Simpson Report that is divided by two to allow for the change to graticule counting, and divided by two again on the assumption that workers will only be exposed to those levels for half the working day.

So that's how the one point two five for fifty fiber years is reduced to one point zero six two five.

So in terms of relative risk, the estimates are identical. The difference in terms of the actual calculated risk is that in the Simpson Report they make the simplifying assumption that a relative risk of one corresponds to ten percent mortality due to lung cancer, which more or less corresponds to...which is death rates.

So a relative risk of one point two five would be a two and a half percent lifelong excess risk in the whole population.

Q. Right.

A. Now, the calculation that I did in the Lancet is in fact...in tab three...is probably rather pointlessly complicated. I looked at the risk in smokers, because I wanted to emphasize the fact that it was restricted to smokers and that would increase the risk by something of the order of

A. (cont'd.) fifty percent...I mean, the risk is virtually zero in nonsmokers, the absolute risk, whether the relative risk is the same...and of the order of three and a half or four percent in smokers, assuming they carried that risk throughout their lives.

Then when I calculated that three percent, I was allowing for the fact that according to this model the relative risk is rising steadily throughout life as the cumulative dose is accumulated, so they don't in fact have a relative risk of one point two five, or whatever, throughout their lives. They have a relative risk which starts at one when they start work, and rises linearly throughout life, reaching a maximum of one point two five at age sixty-five, and then remaining constant afterwards. That's why the relative risk of three and a half ...the excess lifelong risk of three and a half or four percent in a smoker who accumulated that risk instantly at the age of sixteen would be reduced to three percent, because in fact his relative risk is rising throughout his life rather than simply shooting up to the maximum and staying there.

Q. One other matter I was curious about, and maybe you can tell us, why is it in Great Britain when they approached the question of hygiene standards they based them on fifty years exposure, as opposed to the forty or thirty that most people have as a working life?

A. Very few have forty or thirty. I mean, it can certainly be argued that somewhere around about twenty-five would be a more sensible basis for legislation.

I think this is really an historical accident, because the BOHS in their original 1968 report so grossly underestimated the risk of asbestosis, and found that they could guarantee a risk of less than one percent after fifty years exposure at two fibers. They were quite happy to legislate on that basis, and it's really an historical accident as a result

A. (cont'd.) of that statistical error that is perpetuated this has been the basis for industrial legislation.

Q. I take it that you have done a little of your own mathematics with respect to the errors of the 1968 paper, and I think it might be of some assistance to the Commission if you just briefly went into that for us.

A. Yes. In fact the errors in that report are summarized on page 484 of tab three, although they hadn't at that...I was aware of what the results were at that time, of course, but as they hadn't been published I had to present it in a rather hypothetical way.

I've got...do you want to talk about that? I've got the...

Q. If you could.

A. Would you be interested in...I mean, you are presumably already aware of this, I mean the errors in the original study.

The two fiber standard is based on this analysis which was carried out in the Rochdale factory, in 1966. The analysis was a cross-section equivalent study among employed workers who had worked for at least ten years, and they were examined and the proportion with crepitations was based on rales simply observed in the group. So these are simply proportions, one out of twenty, for example, in this cumulative exposure of the order of a hundred fiber years, a proportion of five percent, and so on.

So these are simply...the numbers are written on the graph there...they are simply proportions that were effected in the different groups, exposure groups.

As you can see, there were no cases in the fifty-two people with cumulative exposures of less than a hundred fiber years, and then the incidence rose to something of the order of twenty percent in people with cumulative exposures of four hundred

A. (cont'd.) fiber years.

The data were analyzed by fitting this logarithmic model. The estimated risk at a hundred fiber years comes to considerably less than one percent in fact, although its upper confidence interval is of the order of one percent.

So the basis of these observations, they concluded that the lifelong risk, because these are proportions with symptoms, signs, and the lifelong risk for somebody who worked in such a way as to accumulate an eventual hundred fiber years exposure would be less than one percent. Since creptitations is a symptomatic sign, this seemed a reasonably, in fact perhaps excessively, stringent hygiene standard, and it also happened to be a level which the industry probably could fairly easily accommodate at that point.

So it was accepted immediately and the government at that time in England had no formal mechanism for establishing such standards, and so the Industrial Committee that had made, I think, an honest attempt to evaluate the problem and come up with these results and adopted them as a proposal which they accepted and that seemed acceptable to all the parties concerned, the recommendation was adopted and became law in England, as you know, and since then it has been adopted in most other countries in the west.

It's not generally recognized, but this graph, which I think has a total of sixteen cases of crepitations on it, is the sole formal justification for the two fiber hygiene standard, and there were no other data to support it. This is the basis of the law.

The thing about this population was that they were an employed population, and in fact it was the policy of the factory that when people had symptoms of...early symptoms of asbestos-related disease, to move them from scheduled areas.

So people were in fact only eligible for this study if they developed symptoms fairly recently. So it wasn't in fact

A. (cont'd.) a true estimate of the cumulative proportion of people who had crepitations. It was an estimate of the numbers in these groups who developed them during the one or two or three or four years preceeding the survey.

Another profound mistake with this analysis is that it implicitly assumes that once you have ceased exposure, you cannot develop the symptoms...because if you imagine a situation where the factory were closed down, and then you went back and re-examined the same names ten years later, they couldn't, by definition, move along this axis. If your cumulative dose is two hundred fiber years in 1966, and then you retire, the definition of cumulative dose is the amount of asbestos you breathed in so far. So, by definition, ten years later these seventy-two men with a cumulative risk of two hundred fiber years would still have a cumulative risk of two hundred fiber years.

It's common knowledge that the majority of...or certainly a high proportion if not the majority...of asbestos-related diseases are progressive after retirement, so it's self-evidence that if you came back ten years later and examined the same population, none of these cohorts can by definition move across the axis in this direction, but every one of them would either stay where he was, or move up.

It's simply logically obvious that you went back to exactly the same analysis ten years later, you would get a much higher line.

So that's one fundamental gross error in the analysis.

Another rather serious mistake is that this assumption of a safe threshold and fitting a curve of this shape which presupposes that there is going to be a safe threshold at the bottom, is based on...first of all, it's based on the assumption that such a threshold will exist at all...and secondly, in this case it's based on a failure to distinguish

A. (cont'd.) between ten fibers for ten years, which is more or less actually what this corresponded to in this cohort, and two fibers for fifty years, which is what the legislation was supposed to govern.

In fact, the natural history of asbestos-related diseases are no symptoms or signs are detectable, usually for at least ten years, and then they develop progressively.

In fact, what this graph should be described as is the cumulative...as the prevalence of crepitations in employed men plotted against years since first exposure in which the average fiber levels, according to the estimates that this is based on, would be over ten fibers per ML. That was because... roughly the average level in the factory at the time of this study, and so this in fact doesn't correspond to a hundred fiber years. It corresponds to ten years at ten fibers per ML, and this corresponds to twenty years at ten fibers per ML, and so on. These are, in fact, years across this scale, and this puts a very different interpretation on the graph because what then seems to be happening is in fact, logging it on these safe thresholds, not a hundred fiber years, you've got a lag of ten years before any symptoms occur, and then a linear increase, which obviously puts a different complexion on it, and what you are actually legislating for is to find a prevalence at the end of fifty years.

When you...

Q. Could you just go back over that again, slowly? Just back to the point where you made the translation from fibers per cubic centimeter to years since first exposure?

A. The average dust level in the factory was of the order of ten fiber per ML when this survey was conducted. So in fact, although there was some variation and there were some areas where it was lower, crudely speaking this graph could be interpreted as the prevalence of crepitations in

A. (cont'd.) employed men, ten, twenty, thirty, forty or fifty years after first exposure.

Q. On the basis that a hundred fibers per cubic centimeter, with an average of ten fibers per cubic centimeter a year, would be after ten years...

A. Yes. But this group would tend to have been people who have been exposed to that sort of level for quite a long time. This is obviously a great simplification. I mean the estimates before the war were of the order of fifteen, and the fact that there were points down here and the whole cohort was by definition exposed for ten years, means that there were areas in which the dust levels were lower, but it included approximation and that's what the data would present.

This, represented as a lag of about ten years followed by a linear increase is a very much more accurate description of what the data...of what the whole thing means, and the assumption that a hundred fibers for one years is the same as two fibers for fifty years...which is the assumption underlying the interpretation of the data...in fact this isn't...I mean, some way the assumptions underlying that prediction are that men are not selectively removed from employment when they develop symptoms, which in fact was false in this case. The symptoms would level a bit after exposure ceased, but in fact the majority of them don't, and that's the normal pattern

Q. That's the assumption underlying the 1968 report, and your suggesting the first assumption was in fact false?

A. Yes. And the second and third assumptions. The second assumption is that symptoms don't go on after exposure ceases, but of course they do.

The third assumption is that the low incidence for low doses isn't...the lag effect (unintelligible)...so the analysis

A. (cont'd.) is patently grossly false. In fact, the same study was...this was a cross-sectional study, and afterwards the same cohort was actually followed up and, as I said before, I think that I prefer incidence calculations to prevalence calculations.

If you look at the incidence of, in this case possible asbestosis, which is probably defined in the paper by Geoff Berry in the British Journal of Industrial Medicine, which we referred to yesterday, because these data are presented in there, and in fact they also are presented in an appendix in the Simpson Report, the data that this graph is based on, the incidence of possible asbestosis, which is a more severe symptom than crepitations, in the same population, based on the followup from 1966 to about 1972 or 1973, is on page...

Q. Could I...

A. Page fifty-nine, volume two of the...

Q. Okay, let's all be clear that the data that corresponds to what we've got on the board now is appendix three in volume two of the Simpson Report, which I take it is Mr. Berry's derivation of incidence rates, the second Rochdale study of asbestosis.

DR. UFFEN: Excuse me. How are the cumulative exposures determined?

THE WITNESS: They are more or less determined through exposures which were used after the dust estimates were revised, between 1966 and about 1974/75 when these data were first put together. They were, broadly speaking, the same estimates.

DR. UFFEN: They used an average value?

THE WITNESS: No, no, these are individual estimates.

DR. UFFEN: Individual?

THE WITNESS: Individual estimates.

They are, in fact, the same data that I was given

THE WITNESS: (cont'd.) the first time in the content of the mortality study at Rochdale.

In this case, first of all, the incidence is rising and there is hardly any threshold whatever. Even people with cumulative doses very much below even fifty fiber years, let alone a hundred fiber years, got an appreciable incidence of possible asbestosis.

The most remarkable thing about this graph is the sheer magnitude of the figures. At a hundred fiber years... the previous analysis of this study shows that after a hundred fiber years the lifelong risk for developing crepitations will be less than one percent, whereas this shows that at the end of a hundred fiber years the proportion of people getting it each year was of the order of two percent.

Q. Are you plotting the incidence figures that appear in the second line across in appendix three...?

A. This figure is actually...I don't think the data are subsequently revised...this figure is actually taken from the volume 330 of the Annals of the New York Academy of Sciences, on page 189, the paper by Geoff Berry and Lewinsohn on dose-response relationships referenced to later disease - page 189, volume 330, New York Academy of Sciences.

The methodology and the definition of possible asbestosis are given in detail in that paper.

Q. Just for the purpose of our own record, that's tab number seven of Mr. Berry's compendium of articles.

A. Anyway, if you assume that the dose response is linear, and that the incidence will rise linearly starting at ten years after first exposure, in this sort of pattern, the predictions for lifelong risk of developing even possible asbestosis, never mind crepitations, at a level of two fibers per ML is closer to four percent than one percent.

I mean, it seems that you get a slightly different answer depending on the models that you fit and in that paper

A. (cont'd.) Geoff Berry fitted some slightly more sophisticated models which I'm not altogether happy with, but the main point is that the predicitions are more than an order or magnitude higher than the predictions in the original BOHS study, although it's arguable whether or not you should fit a model which allows elimination of asbestos, and so forth.

But the basic point, again, is ... (unintelligible) ...the risk by a very, very large factor, I think is generally accepted.

Q. Can you just tell us briefly why you are not terribly happy with those various models that Mr. Berry did attempt to fit?

A. Well, the model is fitted is that the effect of asbestos in the lung is weighted by the residence in the lung, and that sort of model, or modifications of it, with powers sort of caught up in cubic powers of time is appropriate for lung cancer and cancer in general, or for mesothelioma, but not, I would have thought, something like asbestosis.

It seems to me that since the data suggest this simple linear incidence pattern...(unintelligible)...the predictions if you do fit it are in fact higher than any of the estimates he puts in this paper.

In the same risks the Simpson Report presented a range of estimates, the highest of which was the most reasonable. His analysis produced a range of estimates the highest of which was actually lower than the predicitions that we made if you just use the simple linear incidence model.

So, I mean, whether or not the models are justified seems to me unanswerable, and they seem to be rather special and unnecessarily complicated, in particular the tables that include the analysis corresponding to the cumulative, the dose analysis which was done in 1968, which is the Rochdale standard.

Q. Sorry?

A. There is a table on page 191 of that paper, which...

Q. Table two.

A. Table two, yes. There is an estimate of prevalence of possible asbestosis corresponding to various assumptions about the duration for which asbestos remains active. The first row of that corresponds to cumulative dose analysis that was done in 1966.

It seems to me silly to have included that when it is demonstrably illogical.

As I say, I think it is argued but obviously not clear that the correct prediction is higher than the adjustments in that table. In a sense it's not there, because I think the highest estimate is actually quite high, and in the same cohort you are producing an estimate of twenty-four percent risk for possible asbestosis when in 1968 the analysis is suggesting less than one percent crepitations under the same conditions of exposure. It illustrates how largely (few words unintelligible) and whether the two figures, twenty-four percent and forty percent when (few words unintelligible).

Q. I'll arrange to have those copied.

A. They are all published, these. I mean, those two slides are published, and in fact the first one is taken from...this analysis is taken from Geoff Berry's paper, I think the 1973 Lyon meeting, on the affects of asbestos, and in fact is taken from the data, they are in the data that were published by the British Occupational Hygiene Society, I think, in 1968 or 1969, which was the original on which the hygiene standard was based.

Q. I just have one or two more questions, and one that I forgot to ask you about your studies at Rochdale, and I just want to be clear about it, is there a reason why, when

Q. (cont'd.) you do your mortality studies that you do not take cumulative dose exposure categories and fit your men into those various exposure categories? Because as I understand it, what you basically have done is taken an average dose per year and worked out, therefore, and average cumulative dose.

We have seen other cohort studies, and I'm sure you know, where they, I take it, try to figure out an individual profile for each person and then get him into a particular dust or fiber exposure category, and then work out dose-response relationships.

A. Our numbers are very small, really. I mean, as I say, I have my reservations about these analyses, as I said yesterday. But, I mean, our numbers are so small that...I mean the numbers for mesothelioma are far too small to analyze in that way, and the total excess for lung cancer, for example, in the earlier cohort, I mean beyond twenty-five years, I think there were fourteen lung cancers compared with seven expected, and the numbers are really too small to analyze in that formal way.

But in any case, as the latest paper shows, the correlation between the individual estimates of dose and risk are zero, so I mean, if..the only thing that we would demonstrate would be a relationship between duration of exposure and risk, which is well known anyway.

MR. LASKIN: Good. Thank you very much for being patient.

DR. DUPRE: Thank you, counsel.

Who wishes to...M. Casgrain?

M. CASGRAIN: I have only a few questions.

CROSS-EXAMINATION BY M. CASGRAIN

Q. I hope you will bear with me first of all because frankly the speed of your elecution is something to

Q. (cont'd.) marvel at. It's about the same as mine in French.

I can't blame you, but you'll have to bear with me if I ask questions to which you have already given the answers.

I would first like to talk to you about Dr. McDonald's study. I would like to understand what you had to say about it exactly, and correct me if I'm wrong.

I think in talking about it the first time, you used tables and you used percentages of the relationship that is between mesotheliomas to cancer. Do you recall that?

A. Yes.

Q. Could you go back to that table? I don't have the tab or the reference to it.

A. I know the figures for his study, in fact, or do you want to see them...

Q. I want to see it with you and try and discuss it with you.

A. Yes, surely.

Q. I think I understood you to say that when you look at Dr. McDonald's study, I think when you take eleven into forty-six, that gives you twenty-four percent. Or did I get... twenty-four percent?

A. Yes.

Q. That's where you get your percentage?

A. Yes.

Q. You are relating the percentages of the number of meso (sic) into cancer, and that gives you twenty-four percent?

A. Yes.

Q. Excesses.

A. Yes.

Q. Then you compare that to the others and that is what permits you to say that in effect chrysotile causes as much mesos of that nature, as crocidolite. Is that correct?

A. Well, what I'm saying is that the effect in terms of causing mesotheliomas is more or less the same as the effect in terms of causing lung cancer, at a given dose effectiveness, not at a given fiber level.

Q. What's...leave aside the fiber level for the time being. So you are saying if you take a proportion...because you find the proportions being the same consistently, this is what permits you to say it?

A. Yes.

Q. Now, does that mean...in my mind I'm trying to figure it out...does that mean that as the number of cancer patients would increase you would expect that the number of mesos would increase?

A. No...

Q. And vice versa?

A. No. As the excess of lung cancer over the expected increases, you would expect that mesothelioma is going to increase...

Q. As well.

A. In point, you see, the absolute excess in Corbett McDonald's study is actually very small for lung cancer. The total number of lung cancers is enormous, but in the insulation workers, for example, who obviously had a much heavier exposure, although the actual number of lung cancers is only twice as high, the excess is ten times as high because the relative risk is so high.

So what I'm saying is that the excess of lung cancer should be more or less proportionate to the number of mesotheliomas.

Q. Could you put it the other way? Could you show that the excess of mesos could be proportionate to the excess of lung cancer?

A. Yes.

Q. If you said that, would that mean...I'm so scared of your answers because you go so fast and so many figures, I get all lost within about two seconds. If you could go slowly... could that mean, perhaps, that in effect you would have a proportionate increase or decrease of both? That's what I'm trying to ask.

A. Yes. Yes.

Q. All right. If that would be so, that would mean...would it or could it...that the meso is dose-response related in the same way as cancer is?

A. Yes, roughly. As I said before, I mean, the effects of duration aren't identical, and in particularly if age of first exposure is very different. But given that...just making the blind assumption that these cohorts are all more or less of similar spread of age at first exposure, and that they contain similar proportions of smokers, and that there aren't vast differences in the average durations of exposure, as a broad generalization, yes. I would say yes, that if you halve the dust level you would halve the excess of lung cancer and halve the number of mesotheliomas.

Q. If I go on from there, could I then say that if I reduced my dust I would reduce proportionately the risk?

A. Yes.

Q. For meso as well as for cancer?

A. Yes, certainly.

Q. In the same way?

A. Yes.

Q. Now, another question I want to ask you, is that the only data on which you would rely to say that Dr. McDonald is wrong when he says that in effect, in his view and from his studies, chrysotile causes far less meso than crocidolite does? Is that the only data you have on it?

A. You have to define rather stringently what

A. (cont'd.) you mean by causing less mesotheliomas. I mean, as I say, in gas mask workers who have worked with crocidolite, the incidence of mesothelioma...I mean certainly anecdotally and I think demonstrably...is very much lower than the gas mask workers who worked with chrysotile.

I would suspect that that's because the conditions of employment were such that the actual fiber levels were very much higher in the crocidolite gas mask workers, so from the point of view of legislation it may be...there may be no difference fiber to fiber in the risk associated with crocidolite, but it may well be...and I think, in fact I would expect that for given industrial processes with the same control procedures the risk associated with crocidolite is likely to be considerably higher than it is for chrysotile.

You see, the main...I'm not quite sure what the situation is in Canada. The importance of this issue in England is not in fact whether we should reintroduce crocidolite, which we are going to do, but whether or not we over or underestimated the risk associated with chrysotile in situations like the Rochdale factory where there was a relatively small proportion of crocidolite.

Q. That is why I am addressing myself to that question, because I think you have also said that in your view the McDonald study should not have been taken into account in setting the standard, because of the fact that maybe the assumptions were wrong.

But the question I want to ask you is the following: I know you are a mathematician. Therefore, you take the figures and you arrange them, draw conclusions from them...I didn't want to use the word arrange...or even the fact that you may be juggling with them, I would not dare say that to you, although sometimes I have been dazzled. But, what I'm trying to ask of

Q. (cont'd.) you, and please try not to deluge me with too many figures in your answer, I am asking you the following question: What data do you have to say what you have stated in this connection, apart from the figures to which you have just referred in that graph?

A. As I said, the absolute incidence rate of pleural mesothelioma in the range of studies which I have tabulated...

Q. While we are at it, could we identify where that particular table comes from so that I can refer to it otherwise than as 'that table'?

A. I don't think it's in any of the papers.

MR. LASKIN: We are reproducing it for you.

THE WITNESS: It is being reproduced.

M. CASGRAIN: And it will bear exhibit number what?

MR. LASKIN: It will have a designation...

MR. HARDY: We gave it a number yesterday.

MR. LASKIN: I can't recall what it was.

Mr. Peto, could you assist the court reporter by...I think it would assist the court reporter, if you are not going to use the transparency, if you were beside the microphone.

THE WITNESS: Do you want me to leave that up for the time being?

M. CASGRAIN: Yes, I think you should leave it there for the time being.

THE WITNESS: A. In table four of tab nine, there is a list of the incidence of mesothelioma...I discussed this yesterday...which looks at the absolute mesothelioma rates in different cohorts. There were differences between these cohorts in duration of exposure. I mean, the amosite factory described in the bottom row of that table...

M. CASGRAIN: Q. Which table are you referring to there?

A. Table four in tab nine.

Q. Yes.

A. The figures which are headed 'relative risk', are in fact measures of the relative incidence of mesothelioma in those different cohorts, and the...as I say, the peritoneal mesotheliomas, the second column, only occurred in American insulation workers, workers in an amosite factory, and workers in an English factory, and all of those had substantial amosite exposure.

But looking at pleural mesothelioma, which is the particular issue that we are concerned with in relation to the interpretation of the Rochdale data and other situations where there has been exposure to chrysotile as well as crocidolite, the actual incidence rates don't vary enormously. They vary by a factor of three between the insulation workers, which curiously have the lowest rates, to the factory workers described by Newhouse and Berry who had substantial amosite and crocidolite exposure, as well as chrysotile exposure, to...there's two point five three in that factory. This is a measure of incidence. There's two point four five, more or less exactly the same, in the amosite factory described by Herb Seidman, and two point nine four, in fact, in the Rochdale chrysotile factory.

Q. This is on a Peto chrysotile textile factory?

A. Yes.

Q. That is the one that we have been referring to today?

A. Yes. I'm not the manager...

Q. This is the one in which you said that the evidence that there could have been crocidolite used in it is anecdotal, to use your expression?

A. No, no. There was certainly crocidolite there, and what is not clear is when it was used and in what quantities it was used.

Q. So when you call it a Peto chrysotile factory,

Q. (cont'd.) perhaps, that wording could be changed?

A. Yes. On the table on the board it says chrysotile plus crocidolite, question mark.

Q. Oh. We should now add a question mark.

While we are on that particular table, may I ask you a question that appears perhaps stupid to you, but why did you not, in making that study, include the McDonald study?

A. The McDonald study? Because he has never published data on the age distribution of his mesotheliomas. He has been asked, but he has never actually produced the man years in successive periods since first exposure, according to duration of exposure, which is what is necessary to do that analysis.

You see, if you took his entire cohort, there is no doubt that the incidence of mesothelioma would be extremely low, if you took all eleven thousand men. But, I mean, that includes people who have worked there for a month or more.

Q. Why do you say it includes people who worked there a month or more?

A. Because that was his definition of the cohort. He took everybody who worked there for a month or more.

You know that the risk of mesothelioma and lung cancer and everything else is low in people who were briefly exposed, particularly if they are not briefly exposed to very bad conditions.

I mean, within his cohort there were subgroups of a relative risk of three or more for lung cancer.

Q. I'm not trying to defend Dr. McDonald, but...

A. No, no. I'm saying it would be very useful to see that subgroup analyzed in terms of observed and expected lung cancers and the distribution of mesothelioma at the time.

Q. Anyway, what I understand, however, is that

Q. (cont'd.) using the figure on the first, which will be twelve A in due course, that would be just more or less the evidence you have to say that he is wrong in respect of saying that chrysotile, as far as he is concerned, there is no evidence that it does cause mesothelioma?

A. I don't think he can have said that, because there have been cases in his crocidolite mines. I think what he said was that in fact the risk was low.

MR. LASKIN: He said it was a rare...

THE WITNESS: A. I don't think he denied that it caused it.

M. CASGRAIN: Q. Well, perhaps I could quote him for you. While being examined by Mr. Laskin here, he was asked the following question:

"Q. I take it you accept the proposition that chrysotile asbestos can cause mesothelioma, and it's simply that the incidence is much smaller, it's a much rarer event...or do you take issue as to whether chrysotile asbestos can cause it at all?

A. I don't want to take issue with it. I think I would be perfectly content to say that it may well cause a small frequenty of mesothelioma. But having said that, I know of no evidence that it actually causes any."

THE WITNESS: A. I find that a very odd statement. I mean if people who only work in chrysotile mines get mesothelioma it seems difficult to see what the explanation is. I mean, there is some contamination with tremolite in those mines, but I mean that's true presumably of all commercially available chrysotile... and I don't think the industry proposes removing absolutely all impurities so as to be absolutely sure it's pure chrysotile, so that we can start the epidemiology again.

Q. Did you participate...I think you did in fact... I have here from amongst the tab that were given to us, something called Discussion Summary, and it says, "Julian Peto, Ontario Cancer Research Fund, Cancer Epidemiology in Clinical Trials Used Oxford, U.K.

MR. LASKIN: Tab eight.

THE WITNESS: A. Yes, tab eight.

M. CASGRAIN: Q. But there is no date on it. Can you give me the date of that paper, please?

THE WITNESS: A. It's taken from the...I think volume two of the Biological Effects of Mineral Fibers, the Lyon meeting.

MR. LASKIN: 1980.

THE WITNESS: 1980, yes.

M. CASGRAIN: Q. All right.

Dr. McDonald was present at that meeting?

THE WITNESS: A. I can't remember. I think he was. yes.

Q. Yes, he was.

A. Yes.

Q. You were present as well?

A. Yes.

Q. Dr. Becklake was present?

A. Yes.

Q. Dr. Gibbs was present as well?

A. Yes.

Q. Did you, at that time, 1980, discuss with Dr. McDonald your views of his findings in respect of chrysotile causing mesothelioma? Did you tell him that as far as you were concerned he was wrong?

A. Well, I have spoken to him about it several times, yes. I mean, I'm surprised at what he said in evidence. I'm not sure that...

Q. Could it be that you haven't convinced him?

A. Well, I mean, as I say, he is not unaware of the fact that mesotheliomas have occurred in people who, as far as he knows, have no exposure to asbestos other than chrysotile mines in Canada. It's difficult to interpret that observation in any way other than that chrysotile can cause mesothelioma. He is also aware that the ratio of mesothelioma to excess lung cancer, and I have been emphasizing this for a very long time, and in fact he has now presented a paper which I think he has submitted to you, pointing this out himself, including his study, in such a table.

So, I find it difficult to understand, and in fact I dont think...I mean, I agree that at face value that's what he said, but I don't think it's his opinion.

I mean, I think he would argue, certainly, that the risk is very much lower, but I don't think he would say that it doesn't cause it. That's not his opinion as far as I know.

I mean, he submitted the paper that he presented at the Coldspring Harbour meeting recently, where he tabulated studies in this sort of way, and included his study and concluded that in general the ratio of excess lung cancer to mesothelioma was of the order of three to four, and therefore this could be used as an index of total excess lung cancer by simply looking at the national incidence of mesothelioma and multiplying by three or four, which is a point that has been made by other people, including me in the past. It's accepted. I mean, that seems inconsistent with the statement of his that you just read out.

Q. He did go on to say that there were tremolite... that he saw crocidolite, at least one bag, in the mines and that dust from this could have had an incidence of mesothelioma.

He did say that...well, I don't want to mislead you.

Q. (contd.) While we are on the subject of Dr. McDonald, would you agree however that if you were to use his data from mines and mills, as opposed to factories, that it would be proper material to use to set a standard in mines?

In Quebec, I'm talking about chrysotile mining.

A. His data?

Q. Yes.

A. Oh, certainly. Yes.

Q. Do you know what the Quebec standard is?

A. No, I don't.

Q. In the course of your evidence you referred to a mac at one point. I heard you use the word mac in respect of standards. Could you tell us what you had in mind when you used the word mac?

A. I don't think I did use it. I don't know what it means.

Q. I heard you say mac Did you mean a maximum allowable concentration, did you mean a maximum number of fibers in any given environment?

You say you didn't use the word mac?

A. I didn't use the word mac . I never heard...I mean, I've never heard the word. I didn't use it. I think you must have misheard me.

Q. Do you know what the word mac means in terms of setting standards?

A. Maximum allowable concentration? I don't know.

Q. Are you taking it from me now?

A. Yes.

Q. Are you saying, Mr. Peto, that I know more about that than you do? That would be setting an awfully low standard for your knowledge, sir.

A. It depends whether we are looking at semantic or scientific entities.

Q. Well, what I had in mind, you have stated in some of your papers that, verbally as well, that in your view the methods for counting fibers have improved considerably over the years, correct?

A. Yes. I don't know for sure. I forget the exact words I used. I mean the methods have certainly changed, and that's had an effect on the results.

Q. I thought I understood you to say that they had changed to such an extent that you could now count with more accuracy. Would that be a fair statement?

A. I think that's my understanding, yes. I mean the fact that you get higher...it depends what you mean by more accuracy. I mean, the introduction of graticule, I think, produces more uniformity and increases the count, so I suppose they are more acurate and more consistent, yes.

Q. That is, counting with phase microscopy?

A. As far as I know, yes, and I think the actual preparation of the samples and the magnification are similar to the methods that we used in the past. I mean, the main difference, is I understand it, is simply the use of a grid so there is a very well defined section of the field in which fibers are included, whereas before there was an ambiguous situation, particularly in relation to fibers on the periphery.

But I'm not an expert on that. I mean, this isn't...you know.

Q. I shouldn't pursue it, but anyway what I want to ask you is this: If I could extrapolate on that particular point, if I may use that expression, if six years or seven years ago I was counting fibers and came with a count of two, not using, for instance, graticule, and using a microscope not as perfect as the one I would be using today, and if I still count two fibers today, I have improved my standards to that extent, have I not?

A. Yes.

Q. So that a standard which would have been set say five years ago, given the fact that there would have been improvements, might be a more prudent standard today than it was five years ago, given the number of particles I could count, is that correct?

A. It might be, yes. I mean, this isn't something that I'm an expert on, and in fact the evidence on it is actually very ambiguous and rather poor. If you look in volume two of the Simpson Report, there were tables there which showed the relationships between counts taken in various ways, and they are really very variable. I don't know really how you can resolve this issue, but I spoke to Bill Nicholson about this recently and he said that in fact the graticule had always been used in the United States, and he wasn't aware of this distinction.

Whether that was true or not, I don't know. I mean, I'm not sure that these conversion factors of two and two point five and five should be applied to all previous estimates and all previous measurements.

Q. Did you discuss it with Dr. Gibbs as well?

A. No.

Q. At that conference to which I referred, was he present and did you not discuss it with him?

A. No. No, I didn't.

Q. I don't suppose you discussed with Dr. Gibbs the fact that you considered that his method of relating dust particles to fiber counts were completely worthless and clearly absurd.

A. But, I mean, that was his conclusion. That was the conclusion of that paper.

Q. It was his conclusion, was it?

A. That was his conclusion. I mean, if you look in the same article by Steel, I think, in volume two of the Simpson

A. (cont'd.) Report, you'll find that the statement... I can't remember the exact words...but in fact that these conversions are virtually indefensible.

Q. Perhaps with semantics.

A. Sorry?

Q. Perhaps we are just doing semantics. I had better wait until I see Dr. Gibbs.

A. I mean, we've got the papers here in fact. I mean, I've got the paper that I took that graph from.

I don't know where it is, but we can see what his conclusion was.

Yes, he concluded that...

Q. What tab is it? I can't find it here.

A. This is a paper by Dr. Gibbs that was published in 1974, in volume twenty-eight of the Archives Environmental Health.

Q. Just one second.

A. He says that, "An investigation in which..."

Q. Would you just hold one second. I'm looking for the paper.

DR. DUPRE: If you could just hold it for a moment, because we want to be sure we have that paper identified. We have a folder for Dr. Gibbs as well and it will save us all time if we can...

M. CASGRAIN: Mr. Chairman...well, I'll wait for Mr. Laskin.

DR. DUPRE: Yes, if you would, please.

M. CASGRAIN: It's tab seven, I think, of Dr. Gibbs...

MR. LASKIN: Six.

M. CASGRAIN: Six?

MR. LASKIN: The one in the Archives?

M. CASGRAIN: Dust Fiber Relationship in Quebec

M. CASGRAIN: (cont'd.) Chrysotile Industry...from Archives Environmental Health, volume twenty-eight, February, 1974.

Is that what you are reading from, Mr. Peto?

MR. LASKIN: You are right, tab seven. This will be tab seven in Dr. Gibbs' compendium of articles.

DR. DUPRE: It will be tab seven?

MR. LASKIN: It is tab seven.

DR. DUPRE: It is tab seven.

M. CASGRAIN: Q. Well, if you look at table...I'm sorry. Perhaps you could go back to my question. I'm looking at the conclusions now. Is that what you have in mind?

THE WITNESS: A. I think the data are more interesting than the conclusions. I mean, the actual ratio of membrane filter counts to midget impinger counts, even if you restrict your attention to mills...so I mean, we are now in a fairly...I mean, we are looking exclusively at observations taken simultaneously, side-by-side, at the same time, in a chrysotile mill. You find the ratios vary from nought point three to forty-seven point four...a factor of the order of a hundred and fifty or two hundred.

So it seems to me that in those circumstances to try and assume that this will give you a ratio which can be applied in different industries is unreasonable. I mean, he concluded in the paper that "no single conversion factor could be applied to all mines or to all work areas within a mine".

So it seems that the conversion is really completely worthless from the point of view of...that was the thing that I said before, that you just quoted.

Q. Well, you said completely worthless and clearly absurd, if I quote you correctly. I took it down.

A. Yes, I think it would be if you took one of these factors. I mean, would you choose to take nought point three or forty-seven point four. You would have your critics

A. (cont'd.) whichever you chose.

Q. Mr. Peto, this article was written in 1974, by Dr. Gibbs.

A. Yes.

Q. Did you have occasion to read any of the material following that, further work he would have done in trying to correlate particle counts with fibers?

A. My understanding was that he had concluded that it was a pointless exercise, on the basis of this analysis.

Q. That's not my question. I'm asking you whether you had occasion to see other work by him?

A. No.

Q. Have you, in 1980, spoken specifically to him...

A. No.

Q. ...about that particular correlation, whether or not it could be properly established? You have not?

A. No.

Are you suggesting that it has been? I mean, if you are aware of any later information that shows that these conclusions are false, I think that the Committee really ought to hear them, because it's a most important piece of information that the Committee be offered, but I'm fairly certain they don't exist.

Q. I'm not brought to testify here, I'm only here to ask questions. If I was to give my own evidence, I don't know what would happen.

A. Cite the reference and ask me if I have read it. Go on.

Q. I would like to go to tab one with you, page 173, in your discussion. Something here, I think...I'm probably not reading it well, but reading here the page in the...

"The object of the study was to establish a value of technical improvements since 1931".

Q. (cont'd.) Then here's what bothers me a little bit on that, because it's very..."but as there is a delay of fifteen or more years between first exposure and any resulting cancer..." you go on to say..."our results do not reflect the effects of working conditions over the last fifteen or twenty years."

Now, should I understand from that that you are taking a period of fifteen or twenty years as being sufficient to indicate, or is that sort of the minimum period?

A. It's usually the minimum. If people are very, very heavily exposed briefly, then you can sometimes see significant excesses before that. But certainly not usually before ten years. I mean the data that I showed on the amosite factory that opened at the beginning of the war showed an excess, in fact, between ten and fifteen years. But that's unusual.

In general, fifteen or twenty years is an absolute minimum.

Q. What are the parameters between minimum and maximum?

A. Well, the maximum is the human life span, as you've seen in Professor Selikoff's data. The incidence of the absolute excess of lung cancer and the incidence for mesothelioma rise absolutely progressively throughout life, in asbestos workers. As I say, the minimum is determined in fact by the intensity of the exposure and the cohort size, and I doubt if you would ever see any excess within less than ten years under any circumstances.

In general, in the sort of environments that we are talking about where exposure is not at a very high level and prolonged, it's unusual to see excesses before twenty years.

There may be the odd case of mesothelioma or occasional studies in which there is an excess of lung cancer between fifteen and twenty years.

Q. Let's go now to tab three, page 488, in which you refer to asbestosis..at the bottom of page 488.

A. Yes.

Q. Last line: "There may well be safe or virtually safe thresholds". This is for asbestosis?

A. Yes.

Q. Do you have any idea of what that threshold should be?

A. Well, the basis that I showed just now, which showed a substantial incidence of possible asbestosis, which is really not very different from certified asbestosis...I mean the criteria are not very much weaker...at levels of the order of thirty fiber per ML years suggests that if there is a threshold, it's fairly low.

Whether or not deaths will be caused at that level remains to be seen. I mean, whether or not possible asbestosis in people who have had low exposure is the same thing as possible asbestosis in people who have had high exposure isn't clear.

As I say, there is no evidence at all. The incidence doesn't simply continue down to zero. But I, just for theoretical reasons or biological reasons, that seems to me very unlikely to be true and so I would assume that there is a threshold at some level.

Q. So you would not apply the linear curve to asbestosis?

A. I think you should actually, because...well, it depends what level you set your hygiene standard at. I mean, in the region of one or two fibers, I would certainly think that what evidence there is suggests that the incidence of symptoms is still linear.

As I say, whether the incidence of deaths caused by asbestosis is still linear is simply not known.

A. (cont'd.) I think in a way it doesn't matter very much, because they don't constitute the majority of deaths that are caused by asbestos, and in a sense, as you've seen, we are arguing about factors of five or ten in trying to determine what the effects of particular dust levels are, so it isn't really worth arguing about.

I mean, I think that the appropriate procedure is to make estimates of mesothelioma and lung cancer and then to say that it's possible that these predictions of risk should be increased by somewhere between nothing and fifty percent to allow for asbestosis, and leave it at that.

I'm not sure there's any point at all in getting involved in formal model fitting for asbestos.

Q. While you are on that subject, I think you also in your papers talk about the fact that one should not have a pessimistic approach so as to actually prevent an industry from actually operating. What did you have in mind when you said that?

A. Well, I mean if you take the study that we did at Rochdale after 1951, for example, I mean it could be argued that there is an onus on anybody who is legislating in relation to public health to impose legislations on the most pessimistic basis because it isn't something that one can afford to be optimistic about.

The upper ninety-five percent confidence limit for our relative risk was nine point seven, which would mean that the excess was more than double the point estimate, corresponding to a relative risk of five.

If you took the measurements as they were taken in that particular study, that would suggest that the relative risk for lung cancer is of the order of ten, which would mean that virtually everybody was being killed by it at a hundred fiber years. I mean, I won't bother to go through the calculations, but you can see that the implications would be that asbestos should be abolished over night.

A. (cont'd.) I mean, I think that would, in the circumstances, I mean given the inconsistency with the previous study and the fact that we know that dust measurements have changed, I think that would be an unduly pessimistic interpretation of those data. Quite where the balance should be struck between optimism and pessimism isn't really for me to judge.

Q. May I turn now to tab five, page 195, and there is a sentence here I've read several times and I still don't know whether I understand it properly. It's the second paragraph on page 197, the second sentence, and it reads as follows:

"It is biologically plausible, there is direct evidence that it is at least approximately true at high dust levels, and there are even grounds for assuming such a relationship for the purpose of setting of hygiene standards when it is known to be false".

I must confess I don't understand that.

A. The thing is that in fact when you impose a hygiene standard, there is very considerable variation about it. I mean, if you impose a hygiene standard of one fiber per ML or point five of a fiber per ML, or point one of a fiber per ML, there will be many occasions when it is exceeded, inevitably, by the nature of things. You won't impose a hygiene standard which is never exceeded.

I mean there will certainly be occasions when the levels exceed that, and it's likely that there will be individuals who will accumulate higher doses. So, for example, you had a situation where the risk was zero up to a certain level, and then increased, if you set your hygiene standard at the point where the line started to rise, then those individuals who had gone above it would in fact receive dangerous doses.

Shall I...I mean, shall I say it again?

Q. Yes, I'm afraid I don't understand.

Q. (cont'd.) What you are saying...are you saying even knowing...

A. I really should say, I mean I think it's important to say in this context that since there is absolutely no theoretical or direct evidence that the responses aren't linear, that I think this is really rather an irrelevant question. I think it would be absolutely unreasonable to assume a threshold in relation to any cancer for the effects of asbestosis, but I mean just as a sort of theoretical interest, I mean the point I was making was that in fact if the dose response goes like that, and you set your hygiene standard here, then in fact the levels that people are actually exposed to won't all be there. They will be spread out like this and so some of them will in fact be exposed in the region where the risk is appreciable.

So that even if you could control peoples' exposure absolutely, and there were an absolutely safe threshold, the way that that would operate in the practical world would be that some people would get dangerous doses.

That's all I was saying.

Q. You know, I think this is when you mentioned mac, you said a maximum. You just said it awhile ago. If you set a standard at the level which could never be exceeded, so that was when you mentioned mac.

A. Yes. I mean I don't know how such standards are imposed, in fact, and I'm not sure whether, you know, the idea is to impose a time-weighted average or to exceed, maximum levels which are never exceeded. Because it obviously makes quite a big difference in the point of view of the interpretation of these data which we are talking about.

Q. In tab seven, page 834, I won't try to look for it now, but my recollection is that you talked about the ambient air background level being not the primary source of inhaled fibers. I'm not questioning what you stated there.

Q. (cont'd.) I would like to know where you get your information from.

A. Well, this is a speculative remark in the discussion of this paper. What I said in that paper, which I think is true, is that, as I say, that there is a weak correlation between morbidity and mortality in measured dust levels, the apparently good relationships that have been observed in various studies are very largely, in fact, correlations between duration of exposure and risk, and not between measured levels and risk. There are various ways of interpreting that.

One is that the ambient conditions in the factory don't in fact vary very much and the high and low sampling results are artifacts, in which case the people who develop the disease are in effect selected at random. I mean, it's fairly obvious that if you took a cohort of people who all smoked twenty cigarettes a day and then took the ones who died of lung cancer and the ones who didn't, and did a case control study, you would find that the smokers smoked exactly the same amount as the nonsmokers, and that wouldn't prove that smoking doesn't cause lung cancer. (sic)

Another possibility, which is what I was talking about there, is that in fact transient high exposures are in fact a major source of risk, and this hasn't been examined in proper detail. It could be very easily. It's simply a matter of doing a lot of detailed measurements, both personal and static measurements, and seeing whether the areas under the large peak actually account for a substantial fraction of the total inhaled asbestos. This is something which I think hasn't been very well studied, and that would certainly be one explanation for this failure.

Another more disturbing one, which I think has to be borne in mind, is that in fact the range of fibers that are being counted is really quite inappropriate, and that there is a

A. (cont'd.) subset of the fibers that are being counted which are the dangerous ones. I think that's quite a plausible interpretation, and I mean conceivably the very long, fine ones, for example...I mean if anybody asserted that the risk is virtually confined to the effects of fibers more than ten microns in length, for example, I'm not suggesting that that's the case, but I mean I don't think there were good data that show it isn't the case, because samples have never been prepared sufficiently carefully for the effects of different lengths and diameters to be analyzed in detail, and that's a very obvious explanation of course for this failure to find good dose-response relationships.

Q. I have one final question, and I approach it with some trepidation, because I...in the course of your evidence you referred to relative risk. You say that to find out about relative risk you first assume or take as a fact the one, the number one as being the normal risk for the population of lung cancer?

A. Yes.

Q. Dying of lung cancer?

A. Yes.

Q. That one, if I understand correctly, happens to be the number that normally die of lung cancer within a year, right?

A. Yes.

Q. So the number you have there may vary from country to country?

A. Yes.

Q. It could be that the 'one' in England gives you a higher number of deaths?

A. Well, I showed a graph actually of lung cancer rates in young people in England, and in Canada and America, and in fact the variation is rather slight. In the United States, although the overall lung cancer rate is much lower than they

A. (cont'd.) are in England, among young people they are likely considerably higher than they are in England. It's quite clear from the epidemiology of lung cancer that if lung cancer rates are high now in people aged forty or fifty, in American and now in England, then they will be higher in twenty years time among people aged sixty or seventy, and so in the future the lung cancer rates in American will considerably exceed the English rates.

At the moment, the Canadian lung cancer rate is slightly lower than the English lung cancer rate, although not by very much. That's Canada and that's U.K., in middle age.

But I think...I'm not sure that they are yet stabilized. I mean, Canada has got a very odd age structure. The population distribution, and your population has gone from five million to twenty-five million over this century, and your lung cancer rates have gone up more steeply than ours, presumably because people have taken up smoking and because smoking has arrived from Europe, and so I suspect that the lung cancer rates are rising in Canada, whereas in fact they are falling England. I'm not sure that that's the case, but I think it may be the case.

So, I mean, I think that roughly speaking, Canadian and British rates can be expected to be similar, and American rates can be expected to be rather higher. So I think the assumption that ten percent of people will die of lung cancer in the future is a reasonable enough approximation, although obviously if you continue to legislate to reduce tar levels, I mean that will substantially make a difference to the risk.

I mean, to the extent that Canadians give up smoking or smoke low-tar cigarettes, then the effects of asbestos will be reduced, reducing measured lung cancer.

Q. All right. My question then is the following, following from this: When you take the figure one, in it...and you talk about people who normally die of cancer, in it you have to

Q. (cont'd.) include to some extent, although I suppose to a very small proportion, those who in any event die of asbestos, cancer due to asbestos. Is that correct?

A. I'm sorry...

Q. To start off...

A. I'm sorry. Could you put that proposition again?

Q. I didn't put it properly. As you start off, and you say one represents the number of people in England, for instance, who will die of lung cancer every year...

A. Yes.

Q. ...and any excess of that, obviously, is your risk, right?

A. Yes.

Q. That's how it's relative?

A. Yes.

Q. But in the one figure, the first one, you have at the outset included, in any event, have you not, those who are going to die of asbestos cancer...cancer due to asbestos?

A. Nationally, you mean?

Q. Yes.

A. Oh, yes. There's a tiny fraction though. I mean...well, I don't want to start bandying figures about...but I mean they are a negligible fraction. They don't substantially affect the calculation.

Q. One last question on that particular line...

A. Incidentally, of course, their inclusion would increase the risk. So I mean, if you excluded the ones that were due to asbestos, I mean the relative risk would be increased.

Q. One last question, it has to do with linearity to some extent I suppose, from what I understand you have an ever constant risk of people dying from cancer, a certain percentage,

Q. (cont'd.) a minimum number of people dying from cancer at any time.

A. Yes.

Q. Lung cancer, that is.

A. Well, no. Lung cancer rates are changing dramatically at different age groups and in different countries. I mean, it's virtually entirely due to cigarette smoking and it's precisely explainable in terms of changing cigarette smoking habits. It isn't constant at all.

Q. Could you, in your experience, tell me which in your experience, in your view, are the...how do I...the products which have been studied the most in respect of their relationship to cancer?

A. Well, really the only ones which are at all well studied are cigarette smoking, asbestos, and I suppose radiation. I mean, the data on cigarette smoking are excellent, the data on asbestos and radiation are pretty poor. I mean, the data on asbestos are better because they have, there are larger numbers in some of the studies, but they are very much worse in relation to dose measurement.

So I mean, asbestos and radiation are both not up to much. Cigarette smoking is excellent. The others are virtually dreadful, and there aren't many human carcinogens which have been well studied.

Fortunately, there aren't many which we can identify cohorts with which, in which there were very large excess mortality rates.

Q. If I understood correctly, are we in agreement that cigarette smoking has been studied the most, and then...

A. Asbestos and radiation, in different ways.

Q. Then radiation coming in third?

A. I would put radiation second.

M. CASGRAIN: I have no further questions.

DR. DUPRE: Shall we take our break then?

DR. MUSTARD: No. I would like to just ask if you know the answer to a very complicated question, I admit, but it's important to our earlier discussion and we may have to try to see if we can get the information from another source, if possible.

In the McDonald study in the Quebec mines, the cohorts were defined in a very different way than in many studies, and you have levels of dust exposure ranging from low to very high, and you also have time of exposure and duration. There is one part in that cohort, in the gross service more than twenty years where the dust exposure is veryhigh, and that's the incidence of malignant neoplasms. It's recorded, but it's not broken down to mesotheliomas in the actual paper.

Do you know of any place where that data has been broken down in terms of lung cancer versus mesothelioma?

DR. DUPRE: For the record, I think Dr. Mustard is referring to tab eighteen in exhibit eighteen, this is the McDonald compendium, and Dr. Mustard is looking at page...

DR. MUSTARD: Page 18, table 7 D.

I suspect you don't know the answer to the question but I thought I might raise it.

THE WITNESS: No, I've asked...I mean, I've asked Corbett for those data. They haven't been made available yet.

DR. MUSTARD: Thank you.

THE WITNESS: It's a shame, because I think that's an important question and a very easy one to answer.

I would have thought that if you asked him for them specifically he would probably provide them.

DR. MUSTARD: My reason for asking the question is, and I would like you to correct me if my understanding is wrong, a characteristic of the Finkelstein study is a very

DR. MUSTARD: (cont'd.) tightly-defined cohort of people with reasonably clear exposure identification to them, whereas the McDonald study is a very broad group of people and you have to dig out the cohort to start to be able to address some of the questions. Is that a bad understanding on my part?

THE WITNESS: Well, you mean it's just heavily diluted with people with low group exposures?

DR. MUSTARD: Yes.

THE WITNESS: Yes, that's right.

DR. MUSTARD: Thank you.

DR. DUPRE: Thank you.

Let us rise until about eleven thirty-five.

THE INQUIRY RECESSED

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THE INQUIRY RESUMED

DR. DUPRE: If we may reconvene, please, the first thing I want to say is that the day on which we next reconvene remains uncertain. However, we shall either reconvene on Tuesday, August 11th, for the usual ten a.m. start...correct, counsel?

MR. LASKIN: Correct.

DR. DUPRE: Or do you want to roll us out earlier than that?

MR. LASKIN: I don't believe I want to do that.

DR. DUPRE: All right. So we shall either... after adjournment today we shall either reconvene on Tuesday, August 11th, at ten a.m., or at two p.m. on Thursday, August 13th.

MR. LASKIN: The Tuesday, August 11th day depends to some extent on the availability of witnesses and so on, but the August 13th day, as I understand it, Mr. Hardy, is fixed. That is when he is calling evidence.

MR. HARDY: That is correct.

DR. DUPRE: August 11th, you will confirm...?

MR. LASKIN: I'll speak to the parties, yes.

DR. DUPRE: And as I understand it, if we were to meet on the 11th, we would not be meeting on Wednesday, August 12th?

MR. HARDY: So we are talking about one possible witness on August 11th?

DR. DUPRE: August 11th, yes. Then on Thursday, August 13th, beginning at two o'clock, following through Friday, August 14th.

MR. HARDY: Right. I think just to clarify for the record, our intentions on behalf of the Asbestos Information Association are to have Dr. Kenny Crump testify on August 13th. He is a biostatistician, and will be talking about the medical evidence.

On Friday, August 14th, assuming we are done with Dr. Crump, to have Harry Rose and Jerry Jakes, who work for Union Carbide and Johns-Manville respectively, discuss monitoring issues. That will be a joint presentation of those two men.

DR. DUPRE: Thank you.

M. CASGRAIN: Do I understand that the 19th and 20th are still scheduled?

MR. LASKIN: For your client.

M. CASGRAIN: For our people. Thank you.

MR. LASKIN: That's right. The only other day that right now is fixed, just to complete August, is Alison McDonald's testimony on August 27th.

M. CASGRAIN: The 27th? Just a minute. I don't see...we are not sitting on the 18th. It's the 19th and 20th, right?

MR. LASKIN: Correct. Having said that though, I think we should advise the parties that, subject to whatever limitations you can, to keep some more dates in August fairly

MR. LASKIN: (cont'd.) flexible, particularly the last week in August. The week that Dr. McDonald testifies I would encourage all of you to keep it relatively clear.

DR. DUPRE: Perhaps it would be helpful to the parties if I told them the total number of days that the Commission is reserving in August, because at least those are...

MR. HARDY: Yes, thank you.

DR. DUPRE: ...we would not be likely to meet on other days. We are holding the course August 11th, August 13th beginning in the afternoon only, August 14th, August 19th, August 20th, August 21st...

M. CASGRAIN: Oh, the 21st?

DR. DUPRE: We are holding August 21st.

M. CASGRAIN: For?

DR. DUPRE: Bear in mind that we have a number of uncompleted situations. Dr. Gibbs, whom we had to postpone, we are hoping to get Dr. Dement. So on and so forth.

So I'm giving you the dates, counsel.

August 19th, 20th and 21st. Then, rolling over into the following week, we are holding, I believe, the 25th, Tuesday, through the 28th...25, 26, 27, and 28.

MISS JOLLEY: That's it?

MR. LASKIN: Sorry, Mr. Peto.

DR. DUPRE: Shall we proceed? Who is next in the batting order?

CROSS-EXAMINATION BY MISS JOLLEY

Q. Yes, Mr. Peto, I would like to pursue a couple of questions on measurement around your Rochdale study, and from the board.

The first question, Mr. Casgrain pursued something from tab seven, on page 833, the second paragraph, and I guess I'm a little confused. It speaks to the issue that the risk of

Q. (cont'd.) lung cancer in your earlier cohort was less than the five relative risk in the later cohort. Is it possible that the fiber dimensions in the Rochdale plant altered? Is that a possible explanation as well?

A. It was suggested to me, in fact, by some of the technical there that the effects of more modern equipment and higher machine speeds could have the effect of producing fine fibers, that you could in fact get a change in fiber dimension as a result of the process.

That could be part of the explanation. The anomaly remains, however, because the majority of the 1933 to 1950 cohort were, of course, still employed in 1950 and 1951.

Q. Right.

A. So the actual periods of exposure would largely have overlapped, so it's still a surprising observation.

So, I mean, that could be part of the explanation, yes. I mean, I suppose my interpretation of it would be that it's a combination of chance, because the numbers are small, possibly some systematic effect of that sort, and conceivably because the earlier part of the study was a so-called retrospective/prospective study and the establishment of records post hoc in that way can be less reliable.

It's conceivable that there was some selective removal of affected people. I mean, it's inevitable that that sort of thing sometimes happens, because in fact employment records, the people who are mainly interested in employment records, particularly the employment records of exemployees, are in fact the medical department, and they are likely to be most interested in people who have worked there for a long time and also in people who have medical effects, and it's conceivable that there was some marginal effect of that sort.

These are all speculations and it may not be an exhaustive list, but it is an anomaly which has to be accounted for in some way.

Q. The second question I have is the fiber adjustment, and I just find it difficult to deal with that a company suddenly adjusts its fibers upwards, and when we are actually dealing with setting standard. Can you help me, do you accept the upward adjustment of fibers that has been presented by the company to you?

A. Well, the two point five for adjustment to the graticule counting in particular, and the personal, the static to personal conversion...I'm really not in a position to comment on it, because as I say, I was given the data in the form of final estimates. I haven't had access to the results of the parallel measurements or the detailed measurements in different areas during the 1951 to 1961 period. So I really can't comment on that.

I mean, it's obviously surprising...it's surprising that the changes should have been as large as they were over certain periods.

But I mean, the adjustment they made are broadly in line with the estimates that are presented in the Simpson Report, which suggests that the effect of using graticule increases fiber count by a factor of two or more.

As I say, I mean, I don't know if you have already had evidence on that. It's a very important question which I can't really comment on.

Q. The issue of, Mr. Laskin asked you about the fifty years, and it does strike me that it is possible for people to be employed for fifty years, although presumably that's not a common thing, but when you set standards you are setting standards, supposedly, in the interests of public health, to protect all of the workers in workplaces, and presumably you would take fifty years as the outside limit, wouldn't you?

A. I would have thought it was too long. I would have thought thirty or thirty-five was more reasonable, but I mean

A. (contd.) that isn't really for me to say.

Q. The four hours a day issue, cutting it down again, when you are setting public health standards or occupational health standards is that a fair removal...or reduction?

A. I don't know. This is discussed in the Simpson Report. I mean this is where the factor is introduced. As I say, obviously it depends on the conditions in the factory and the variations of the extent to which people move from one area to another, but in particular it depends on the way that the standard is imposed.

I mean, if it's an average in areas where people work for most of the time, then it shouldn't be applied. If it's a maximum in areas in which people don't spend all their time, then it might be an underestimate of the effects.

I mean, if the measurements were taken in the same way as the original measurements, of course, it would be inappropriate to apply it if the standard were going to be applied as an average over time, because that's exactly the form of the measurements that the calculation is based on.

Q. Yesterday you presented evidence about chrysotile having preferential migration patterns to the pleura. Does that account for the lower amount of chrysotile found in the lung?

A. Yes, both happen. You get preferential migration to the pleura, and removal from the lung.

Q. Right.

A. I mean, I think it has been shown that people who are known to have substantial chrysotile exposure often have very little chrysotile in the lung parenchyma when they are examined after retirement. There is a relatively quick removal of chrysotile from the lung parenchyma.

This makes historical studies very difficult. It would be very nice if one could simply do autopsy studies on

A. (cont'd.) people who had worked in these factories in the 1950's, and then retired. But in fact that won't answer the question.

I suppose it raises a possibility that examination of their pleurae would give you better information.

Q. That hasn't been done?

A. I don't know. For a long time most of the work that was done in England was done on lung parenchymae, which I think was a mistake. I think more recently they have started to look at the pleurae, but I don't know what stage those studies are at.

Q. I just wanted to ask you two short questions about the nonexposed mesothelioma you referred to in the Los Angeles County study, and it seems to me that there are possibly two problems there and you mentioned the whole issue of perhaps misdiagnosis of mesothelioma in that group, and that when they were reanalyzed it was not a blind study. Is it possible to do a blind analysis of the mesothelioma for diagnostic purposes?

A. Yes. I think that is going to be done by the group in Los Angeles.

Q. Is it?

A. In a sense, of course, it isn't an issue that particularly concerns the Commission, I mean whether there are two hundred cases a year in America and perhaps twenty-five a year in Canada which are unrelated to asbestos exposure is a question which may be of medical interest, but it's completely irrelevant from the point of controlling asbestos.

Q. I guess the question is, are they really unrelated to asbestos? Can you determine that by interviewing next of kin?

A. Well, as I say, I would suspect that they are, because the...partly because of the age distribution, which I suppose is a rather sophisticated epidemiology argument which

A. (cont'd.) might not appeal to most people. But also because most cancers occur naturally, and you know, there aren't many cancers which occur exclusively in relation to...as a result of exposure to carcinogens.

Therefore, in a sense, it would be surprising if they didn't, and it's simply a matter of measuring incidence.

Finally, as I say, the fact that the incidence was the same in men and women, I think, tends to support that interpretation.

I suppose I should say that the...both the equal incidence in men and women and in fact that age distribution will also be predicted if they were the result of ambient exposure which people had been exposed to, both sexes are exposed to from birth.

Q. The final question I have, and you mentioned in your testimony yesterday that the fibers found in schools were shorter. I just wondered on what basis you made that statement?

A. This was anecdotal. I mean, Sebastien, the French electromicroscopist, told me that they were of a completely different size distribution from the fibers that are found in industry, but I mean I haven't seen detailed data on it, and I wouldn't like to be quoted as an authority to the fact.

MISS JOLLEY: Thank you very much, Mr. Peto.

DR. DUPRE: Thank you, Miss Jolley.

Mr. McNamee?

CROSS-EXAMINATION BY MR. McNAMEE

Q. Yes, Mr. Peto, in your tab nine, you are talking about experiments with implantation in rats and talking about fiber size. I believe this is page nine. I understood from that that fiber size was quite important to the production of mesothelioma, and have there been any efforts in testing with

Q. (cont'd.) rats to...especially with, say, glass fibers, to standardize the samples being injected into the rats so that you can test different sized samples, different diameters and lengths?

A. Well, I'm not sure that any more detailed experiments than the ones that Stanton did at the NCI, which are published in 1977 in the JNCI, have been done. He tested thirty-four different samples which were in fact very different. They varied from long, thin, short, fat, and there were various gradations in between.

But the difficulty was that they weren't finely enough separated and so it wasn't possible to unambiguously define the risk associated with fibers of a particular length and a particular diameter.

Of course, the other thing is, and the problem is further complicated by the fact that they were pleural implantation experiments and the actual migration to the pleura and then from the pleura to other parts of the body is also largely, and probably entirely determined, by fiber dimension. So fiber dimension is likely to be critical both in its carcinogenic potential when it gets to the target cell, and in its chance of reaching the target cell.

I don't think that there are better data than those.

Q. I understand that mechanically it is possible, at least with glass fibers, to grade the size of the test sample, that then it is possible to reduce with glass fibers these things down to a specific size...

A. It hasn't been done, as far as I know. It hasn't been done...or if it has been done, I mean large experiments haven't been reported where there really is a good division of a relatively small range of length and diameter.

Q. Then I'm in error in assuming that it was mechanically possible to reduce these fibers to different size in tests?

A. I'm sure it's possible. It's just an expensive technical procedure. But I'm sure it is technically possible to do it. If President Kennedy had declared in 1960 that it was going to be done by 1970, it would have been.

Q. In your Los Angeles study, with respect to the background of the mesothelioma incidence, did you check into the working environment of each one of these people? Not only the work environment which was asbestos-related, but past work experience and subsequent work experience? Did you have a full employment profile of your Los Angeles cohort?

A. Oh, yes. Yes.

Q. Inasmuch as...with respect to Dr. Finkelstein's study, he had four people, I think, in one table, of fifteen that had exposures less than four years at a Johns-Manville plant, and one as low as one month. Would it be...you, in doing such a study, do you think it might be important...especially with say the one month...to examine the other employment and background information concerning that person?

A. In a study of that sort?

Q. Yes.

A. Yes, it is a difficulty with studies like that. I mean, I don't see how you resolve that. I mean, it's obvious that the shorter the exposure is, I mean the more one suspects that there may have been previous exposure. But all you can do is ask. It is never possible to be certain.

Q. Yes. But I was thinking just for the purpose of, say, compensation, that a short-term exposure might warrant a closer look into the background of the person who has suffered the disease in one month or one week?

A. Yes, I think that would be fair. Although it seems to me in relation to compensation if you couldn't identify any previous employment which was likely to have entailed asbestos exposure, then it would be reasonable to attribute it to this one.

In a sense, duration of exposure is more important than duration, because the duration effect is enormous and I mean, I wouldn't, for example, regard it as reasonable for somebody to get compensation from the employer when his first exposure had been within the last five years.

I mean, that would seem to be quite unreasonable. If the exposure had started within five years, I would not believe that that was in fact responsible for the mesothelioma.

There was one case, a woman in fact, in our Los Angeles study, who had been first exposed, reported first exposure less than three years before the mesothelioma developed. Well, in fact, we didn't examine the women in detail, but I mean we just described an outline in the discussion, but I would be virtually certain that that case wasn't caused by that reported exposure.

Q. Would you examine what has been called a household exposure or the bystander exposure? Say the parents might have been...one of the parents might have worked for an asbestos company.

A. Oh, there were three women who fell into that category, whose husbands or fathers had been asbestos workers. It wasn't one of them.

Q. If we might ...tab eleven, the study of the trends in mesothelioma incidence in the United States and the forecast epidemic, if you might turn to table seven, which is Overall Projections of Numbers of Mesotheliomas Diagnosed in the United States in Successive Periods in Men First Exposed to Asbestos Before 1965, or Unexposed.

I think this is really...table six bears on the

Q. (cont'd.) point too...when I go through table six and table seven, you seem to have absolute numbers postulated for every five year group, and bearing in mind...well, I assume that the raw population in the United States has increased in between 1981 and 2019, the last date that you have, and also, you have the life expectancy at birth increasing. Now, these are two variables. Are they somehow factored into those numbers, or how do I account in reading this if the population of the United States if three hundred and fifty million, say, in 2019, by reading these I would think the incidence, even if the numbers had remained the same, the incidence had dropped.

Do you understand what I mean by that?

A. Yes. There is a note on page eight of the paper, it's handwritten, it's been added in proof, which points out that the numbers of unexposed cases would in fact increase because of the age distribution of the U.S. changing.

But the asbestos-exposed one won't, because the method of analysis wasn't in fact population-based. It was case-based.

The ...do you want me to talk about it for a couple of minutes? I mean, just to outline what the methodology was? The idea is basically...

Q. It might be too complicated.

A. Well, no, it's very simple actually, an outline. All you do it you...I mean if the incidence goes up as T cubed, and I think I may have showed the graph, if you were born in 1900 and you were first exposed in 1920, then your incidence goes up as T to the three and a half from that time onwards. You don't know at what level, but I mean the incidence rises in that way.

So if you take the entire cohort of people in the whole of the U.S. who were born in 1900 and first exposed to asbestos in 1920, you know what the survival curve is for people born in 1900, that ought to be modified because asbestos workers have rather high mortality, but that's a second order effect.

A. (cont'd.) If you multiply those two curves together, you get a pattern which shows the number of cases that are going to be diagnosed in each year, you see, multiplying the incidence by the population at risk, and that gives you the number of cases.

This isn't to do with the population. This is shape of the pattern of cases diagnosed in 1940, 1960, 1980, in people born in 1900, first exposed in 1920, and you know exactly what shape the curve will be. The only thing you don't know is how high it will be. So you do a survey, you interview people and ask them when they were first exposed to asbestos, and one point on that curve fixes the rest of it.

So the idea is that you do a survey in a certain area and find what proportion of cases, the numbers of cases that that are first exposed...born in 1900, first exposed in 1920, and that fixes...I mean, let's say you got that answer instead of that answer. Then you have this curve the same shape and sort of moved up to go through that point.

So the predictions in relation to asbestos exposure are independent of the population structure, I mean the size of various birth cohorts and the number of people exposed implicitly is allowed for, so the predictions are correct in relation to asbestos exposure.

But as you say, I mean as is pointed out on page eight, an adjustment should be made for the unexposed cases, but I didn't bother to do that because it didn't seem...I mean it wasn't really the point of the paper and it seemed a pointless complication to go into.

Q. Well, this standard format used in these...and I haven't noticed it in these other studies...in which there is a prediction of future incidence, is this something that an epidemiologist takes into account, say an aging population with a mean age moving progressively higher. You know, at one time

Q. (cont'd.) there used to be twenty-five in Canada and the United States, and I think it's now twenty-nine or something like that, plus the fact of an increasing population, is this something that all epidemiologists take into account in these numbers?

A. Where appropriate. Yes. As I say, in this case it's bypassed by the methodology. It's a rather peculiar method of analysis. I mean, there aren't many diseases that you can do it for.

Q. Just to go back to your table seven, and I suppose this is the difference between looking at something prospectively and retrospectively, as I see your numbers, right now in 19...say between the period 1979 to 1984 you have a gross total in table seven of forty-seven hundred and fourteen cases, increasing to fifty-six hundred and fifty in 1989, and that's the high water mark of your epidemic, is that correct?

A. Yes.

Q. So in effect, one way of looking at it is the epidemic is already over, and we are looking at increased...I mean if you discount the latency period...

A. The cause of the...

Q. ...the people have already been exposed.

A. Yes.

Q. Okay. A person who now, from 1981 on, first comes into the risk area, are first exposed, from now on his absolute risk is lower? Or relative risk?

A. Hopefully, yes. I mean conditions are presumably...I mean now are certainly very much better than they were in the past.

I mean, these projections are restricted to the effects of exposure up to 1965, and in fact the data, even from 1950 onwards, is very, very strong. If you look at the actual numbers that these are based on...I mean this paper

A. (cont'd.) is as much methodological as predictive.

I mean, in that survey we had a total of, in table two, there were a total of five cases first exposed in 1955 or later.

Well, I mean, in the future those late periods of exposure will eventually constitute the majority of cases that are going to occur, and these inferences are based on the existence of those four cases in our survey. So I mean, it isn't at all clear what the pattern of exposure was from 1950 onwards.

Our data suggests that in fact it didn't change very much, that there wasn't much improvement from about 1945 to 1965, or perhaps even later.

But they have to be regarded as provisional. I mean, my main aim in life in this paper was to persuade the NCI to do a bigger national survey and get more accurate estimates.

Q. I would assume that, say taking a twenty-five to thirty-five year...I'm using the term latency period again... that I go from 1981 into, say, 2014, which is thirty-five years, say in that period from 2009 to 2014, cases are being reduced, it would seem to me that an implicit prediction, that based on current levels that mesothelioma cases are going to drop. Is there an implicit prediction in that?

A. Well, I'm sure that's the case. The question is how much they will drop.

As I say, these predictions relate to exposure before 1965, because we have absolutely no data beyond 1965, and the observed and expected numbers are both zero, whatever level of exposure you assume, because it's too soon to see the excess.

Q. Okay. You have indicated in this paper too that you might be...there might be a margin for error two times either way. I mean, these figures could be increased by... like say in the last column, 2019 could become 4200, or else could

Q. (cont'd.) be 400?

A. I'm sorry.

Q. Somewhere in this paper, I think, you allow for a...

A. Yes. Sorry, which...?

Q. I didn't know whether that applied to all of your...

A. No, that was in relation to the numbers of cases occurring in people who are first exposed in 1950 or later. In fact in table five there were a total of eight cases first exposed up to age forty-five, first exposed in 1950 or later, and that eight was simply based on those confidence limits for eight, and if you observe eight cases, the right answer could be anywhere between about four and sixteen.

It's not the...

Q. Then these figures you are predicting, what is the range of probability then, if I might...what kind of a factor could I say...you are estimating say five thousand as of 1999, in a five year period, a thousand a year?

A. The predictions in the near future are actually very reliable, because the method was to restandardize internally, so that the estimate equals the observed number that is now occurring.

So it makes no difference over a short period. I mean, if you are observing a certain number of cases of mesothelioma now, per year, and there are something of the order of a thousand a year in the U.S., then it's almost bound to be roughly a thousand next year. But I mean, it may make quite a big difference to your predictions in twenty years time, so in fact the predictions in the short term are very accurate and they become increasingly unreliable as you go forward.

As I say, that particular remark was a purely statistical remark in relation to the sort of standard error of

A. (cont'd.) prediction based on the number eight, the eight cases.

But I mean, there is a prediction in that table relating to the people first exposed in 1960 to 1964. Well, we observed one case in people first exposed from 1960 to 1964 in our survey, so the confidence limits for that are fairly wide.

Q. I just have a couple of questions with respect to your school assessment, tab ten. We've had some evidence from various other witnesses about school programs in Wisconsin, and also New Jersey, and also we have a school program in operation in Ontario.

Are you aware of the nature and extent of the Ontario school rejuvenation program, if I might call it that?

A. In that line, yes. I mean, I don't know the details of it, but I know that a lot of money is being spent.

Q. Yes. I was just wondering, you have had some experience in California, and it has been my observation that California usually leads the nation in a lot of these things.

What is the...could you tell me what the perceived risk is in the schools in California? I mean, if there has been any public statement, and what steps are proposed in California if there is such a perceived risk.

A. No, I'm sorry, I don't know.

Q. You don't?

A. I haven't worked in California for two and a half years, and I don't know in detail what's going on there now.

Q. Obviously with the number, with the sparsity of the cases of mesothelioma per year, maybe seven hundred out of a thousand are because of direct asbestos exposure, we are looking at only three hundred cases that are, say, nontraceable, that it's obvious from that that any study of schools, no matter

Q. (cont'd.) how you construct it, in order to determine whether there is increased risk would be very difficult. Is that a fair statement?

A. Well, do you mean...

Q. Could you ever let excess risk...

A. Could you actually observe mesotheliomas among school children? No. It would be completely impossible. You couldn't distinguish them. The numbers would be so much lower than the numbers that occur apparently unrelated to asbestos exposure, and as I say, they would also occur at the same ages. I mean, they wouldn't occur...in fact they would occur at exactly the same ages.

I mean, if the incidence goes up as a cube of age in unexposed people, and it goes up as a cube of time since first exposure and you were first exposed when you were a child, the age distribution would be exactly the same and it couldn't be determined...I mean, things like that can only be determined by model fitting and estimates of risk based on dust measurements in schools.

Q. So would it be fair to say that it is absolutely impossible, based on present technology, to quantify the...to empirically quantify the risk to school children?

A. No. I think you could do it by looking at lung samples. I think it would be useful to look at numbers of fibers in lung samples, and if you could compare them with figures in recent employees in asbestos industries where measurements have been taken, then you would get...if you looked at current employees particularly...and accidental deaths among school children...you would still have a terrible difficulty because there is great heterogeneity between schools, of course. I mean there are several orders of magnitude difference from low levels to zero levels in the amounts of asbestos in the schools.

A. (cont'd.) But if you wanted to, in a particular incidence, I mean you could in principal look at the lung burden of school children. I think the electron microscopy is sensitive enough now to estimate fiber levels as low as that.

Q. It occurred to me, and I'll just throw this suggestion out, that three hundred...if you analyzed say the three hundred deaths a year that are nonasbestos-exposure related, selected out the three hundred of the thousand that are not directly related...

A. I'm sorry. Which paper are you referring to?

Q. I'm sorry. I think you used the term...whether it was nine hundred a year, that seven hundred were direct asbestos-exposure, something like that?

A. Oh, yes. Yes.

Q. Maybe two hundred.

A. Yes.

Q. Now, if you selected over a term of say the last five years, out of the two hundred, and selected out the school teachers and the maintenance workers or caretakers, would it be possible...I imagine it would be possible to select out those and then examine them to see if you can find any significant numbers. Would it be possible to do something like that?

A. I don't know what you would compare them with, you see. Because the difficulty is that everybody has asbestos fibers in their lungs.

Q. Right. True.

A. In fact when I...

Q. I'm talking about mesothelioma victims. If there was an inordinate proportion of teachers in relation to their proportion of the general population, wouldn't that be something significant?

A. I think it's extremely unlikely, because there are so many other environments where there is minimal asbestos

A. (cont'd.) exposure, I would have thought that teachers, if anything, would have a lower-than-average amount of asbestos in their lungs. I would have thought it was a dead loss.

Q. I thought so myself, but I just...that's why I asked the question.

MR. McNAMEE: Those are my questions, thank you.

DR. DUPRE: Thank you, counsel.

Mr. Hardy?

MR. HARDY: Yes, sir.

CROSS-EXAMINATION BY MR. HARDY

Q. Mr. Peto, I think I would like to clarify first the discussions you had earlier this morning with M. Casgrain about your observations concerning the relative biological significance of crocidolite and chrysotile with respect to mesothelioma, and the first question is just what data you have looked at in reaching the conclusions that you have reached on that issue which you presented the last day and a half?

Do I gather correctly from a question, I think from Mr. Laskin, yesterday that you haven't looked in detail yet at the Dement study with respect to the issue of crocidolite and chrysotile for mesothelioma?

A. No, I haven't, no.

Q. As I understand it, that's a plant which was primarily, if not exclusively, chrysotile exposure?

A. I think there has been one mesothelioma in the cohort, hasn't there?

Q. That's what is reported in that paper, correct?

A. Yes.

Q. All I'm asking now is, you haven't taken the time yet to see whether the data in that cohort fits the same

Q. (cont'd.) pattern in the cohort you presented to us?

A. Well, it does in fact, because the total excess of lung cancer is of the order of eighteen cases. The followup is short. I mean, there is very little observation beyond thirty years after first exposure, so the remarks that I made earlier would apply, that in fact this is a period in which you do see a larger excess...initially you see a larger excess of lung cancer than of mesothelioma, at the beginning of the study.

If there were something like a five-to-one ratio, four-to-one ratio, something of that order, with an excess of eighteen...even leaving aside the point that the ratio would tend to increase as time passes...you would expect to see something of the order of perhaps three mesotheliomas in the population, well, if you expect three and observe one, that doesn't begin to differ significantly.

I think this is something that Corbett McDonald should have discussed in more detail when he presented his comparison of these ratios, because the numbers are so small that statistical fluctuation is actually very large. To get one study in which you get two, and another study in which you get six, as to say there is a difference in the ratio of three is unreasonable. I mean, the more reasonable interpretation is that you expected four in both cases and one happened to be a bit high and one happened to be a bit low.

So I don't think that that study is in any way out of line with the other ones, certainly numerically.

Q. Have you looked at the most recent study, Newhouse and Berry, of the friction material plant in England, with respect to this?

A. Yes, did they...

Q. I know Geoff Berry discussed it when he was

Q. (cont'd.) here before us.

A. Did he present it as part of his evidence?

Q. Yes, he did.

A. You haven't got a copy of it, have you?

MR. LASKIN: Yes, we do. It's tab fifteen of his materials, just for the record.

THE WITNESS: Yes, okay.

MR. HARDY: Q. Had you looked at that study with respect to this issue before, or are you just looking at it now?

THE WITNESS: A. I haven't really looked at it now. I saw a preliminary report on it. I haven't seen it in any detail before.

Q. Similarly, have you looked at the studies, just reported, of three factories in the United States, by Alison McDonald?

A. No.

Q. Helsinki.

A. No. I don't know about those.

Q. I don't really...

A. If you want me to comment on this study, it's a bit difficult to do it without...

Q. I think it is a bit difficult. I really did not expect you to comment. I just wondered whether you had considered it prior to today and as part of reaching your conclusion on chrysotile versus crocidolite.

A. Well, there are two separate issues in relation to this study, and one is whether or not the ratio of excess lung cancer to mesothelioma is anomalous.

Q. It's very low, I gather?

A. What, the..?

Q. The excess of lung cancer is very low in that study to begin with?

A. Is there a table which shows the death rates by times of first exposure? Because...where is that? I can't see a table that breaks it down by times of first exposure.

Oh, yes. Yes, there is an excess of thirteen lung cancers among men, and...no, there are seven mesotheliomas and six lung cancers, in fact, in excess of six lung cancers, beyond ten years after first exposure.

No.

Q. I think with respect to chrysotile versus crocidolite, as discussed in that paper and by Geoff Berry when he was here, it gets complicated because it's a plant in which crocidolite was used...

A. Yes, I know.

Q. ...at one time, in one defined area.

A. Yes.

Q. So that unlike the Rochdale plant, they are able to attribute crocidolite exposures to particular persons in the plant, and it's that analysis which I think led Mr. Berry, at least, in interpreting this study to find crocidolite responsible for most of the...unduly responsible compared to chrysotile.

A. Yes, I think...

Q. I wondered whether you had reviewed that data?

A. I think it's a statistical error, in fact. The point is that the people who were exposed to crocidolite, the area of the plant where crocidolite was used, those people were almost exclusively production workers who had extremely heavy exposure to chrysotile, and so it points out in the paper that in fact if you adjust the apparent crocidolite effects in the case control comparison for the exposure to chrysotile, it's severely weakened.

Now, it's characteristic, if you measure something badly...I mean, I'm not criticizing this study...I mean I think the

A. (cont'd.) dust, exposure measurements in this study are probably very good, but as everybody in this room knows, any estimates of exposure, particularly before the war, are not good. The classification in this was, in terms of chrysotile exposure was whether the average exposure had been to levels of greater or less than five fibers.

Now, the great majority of people which had been exposed to chrysotile...sorry, to crocidolite, had been exposed to levels greater than five fibers. Well, a crude dichotomy into whether or not you are exposed to a level above or below five fibers is obviously not an accurate way of classifying your exposure.

If you find a strong relationship between whether you worked in that part of the plant and your chrysotile exposure, then one would assume that if you estimated the chrysotile exposures more precisely you would find that they were numerically higher, even, than the controls who happened to be exposed to levels above five fibers.

I don't know if I ought to talk about that. It's a slightly statistical point.

The point is that if something is correlated with something which causes something...I don't happen to think of a good example...I suppose social class is a good example, in fact. I mean it's absurd that social class should determine cancer rates, but certain cancer rates are very strongly related to social class and it isn't always possible to get rid of the association.

One doesn't conclude that the lower social classes are genetically more susceptible to cancer than the upper social classes. One assumes that there are environment differences that you haven't measured precisely.

So if you had the hypothesis that being an industrial worker caused chronic bronchitis, and the competing

A. (cont'd.) hypothesis of being of low social class caused chronic bronchitis, you find that social class, social classification still predicted chronic bronichitis datas after you had allowed for the sort of occupation that somebody was working in...in broader terms. I mean, it's precise in the social class specification. Well, the correct inference to draw from that wouldn't be that the cause of chronic bronchitis is social class, which would evidently be absurd.

The reason is that you can't in fact measure the determinance of social class sufficiently accurately, and the thing that is missing from this study is the reverse analysis, of course, because there is a very, very strong correlation between crocidolite exposure and risk of mesothelioma, which is enormously weakened...and in fact I think becomes almost...I'm not sure if it remains significant, but that's really beside the point...it's very much weakened by adjustment for whether or not people were heavily or lightly exposed to chrysotile.

The analysis that I would like to see carried out on the same data is an analysis assuming that the chrysotile exposure caused mesothelioma, which it's quite clear would be significant if you ignore the crocidolite, and then to see if you could completely remove that association by allowing for crocidolite exposure, and I'm sure that you would get completely symmetrical results.

You would find a strong correlation with either. If you analyzed it by itself, it could be weakened, but not removed, when you adjust it for the other.

Q. So then it is your belief that the analysis of this data from this plant would show heavy chrysotile exposures...

A. It does...

Q. ...would be responsible for the mesotheliomas?

A. No, I don't think it would prove that. It would show...I think it would give...it would be as persuasive. It would show that to the same extent that it shows the crocidolite exposures were.

This is rather a complicated issue. I don't know if I...I mean, it's quite an important point and it's worth going into in detail. I mean, there are many examples of this.

I mean, let me think, smoking and birth weight, for example, is one. There is...smoking, birth weight and social class are intercorrelated in this sort of way. Whether a mother smokes or not, and the birth weight of her children, are going to correlate in this sort of way, and when you adjust for one, you reduce the affect of the other, but don't completely remove it. It's a well known difficulty when you measure things fairly well, but not perfectly, and things are intercorrelated, and you really can't sort out what is in effect.

As I say, it's rather obvious, given that these are competing hypotheses, that this table ought to contain the opposite analysis, given that they have classified people (a) in terms of high or low chrysotile exposure, and (b) in terms of whether or not they worked in that part of the factory.

They should have done the analysis in terms of mesothelioma case-control comparison, assuming that (a) causes it, and then adjusting for (b), and then assuming that (b) causes it and adjusting for (a).

As I say, I'm fairly certain that if they had done that, they would have got completely symmetrical, and therefore completely ambiguous results.

Q. Is it fair to say in your discussions of chrysotile and crocidolite and their relative ability to cause mesothelioma that you are fairly convinced first that there is evidence that chrysotile can be responsible for mesothelioma?

A. Yes, yes.

Q. Beyond that, you seem to be less convinced than others that there is a differential significance in terms of causing mesothelioma between chrysotile and crocidolite.

A. I don't know any data on it at all. As I say, I don't think thereis a single study in which either lung cancer excesses or mesothelioma rates have been estimated in people whose crocidolite exposures were estimated in fibers. I don't think there is any evidence one way or the other from epidemiological evidence to indicate whether one is more or less dangerous than the other.

It seems to me that, as I said before, in view of the animal evidence and the human evidence in terms of preferential migration to the pleura and so on, that that effects of asbestos in terms of causing mesothelioma are entirely determined by physical characteristics of the fibers, and that types of chrysotile which are long and fine, which I understand are particularly likely to be produced in certain manufacturing processes more than in mining, are likely to be as dangerous as comparable crocidolite fibers, and as I have also pointed out, there aren't the enormous disparaties between different studies which are claimed. They are in fact simply not there. I know many of them are artifacts of incorrect analysis or failure to allow for time since first exposure, and so on.

So I think that the argument, the sort of popular notion that there is this hundredfold difference is just based on very weak evidence.

I wouldn't...I certainly wouldn't assert that there is no difference between them. But, I mean, the statement is... but I wouldn't assert that there is either, and I certainly wouldn't assume that the difference was enormous, unless some better evidence is presented.

DR. DUPRE: I want to make, for the record, something clear.

DR. DUPRE: (cont'd.) Do you differentiate between pleural and peritoneal mesothelioma? I had the feeling from reading I did that where peritoneal mesothelioma was concerned, your findings indicated that exposure, if not to crocidolite at least to amphibole, was more often associated with peritoneal mesotheliomas than exposure to chrysotile.

THE WITNESS: Well, chrysotile has..nobody who is mainly or exclusively exposed to chrysotile has ever had a peritoneal mesothelioma, as far as I know.

As I said before, I mean, there's an anomaly in relation to crocidolite, because there are groups apparently only exposed to crocidolite who have had large numbers of pleural, and no peritoneal, mesotheliomas. And there are other groups where the numbers have been roughly equal.

Which in a sense reinforces the point that I was making earlier, that in fact it's fiber dimension rather than the actual name, the label that has been given which should be looked at when you are trying to work out what the physiological consequences of these things are.

In people exposed to amosite, I think without exception that there are substantial numbers, substantial proportions of mesotheliomas have been peritoneal.

In Selikoff's data, the majority...but that may be because I think he has actually reviewed his causes of death more carefully than other people, and he has shown rather clearly there's a high proportion of mesotheliomas, peritoneal mesotheliomas, peculiarly likely to go undiagnosed. Yes, and he presents a set of best estimates and death certificate data in his population, and I'm quite satisfied because of the age independence and the fact that they followed exactly the same pattern as the pleural ones, that these are mesotheliomas and that would tend to go undiagnosed in other studies.

So it may be that amosite causes a higher risk

THE WITNESS: (cont'd.) of peritoneal than of pleural.

DR. DUPRE: Again, this would be because...if this is so...this would be because of the dimension of the fiber?

THE WITNESS: I would assume so, although...yes, I mean I don't know much about the chemistry of it. There are chemical changes. I mean there's some leaching of magnesium, and so on, from the surface of chrysotile, I think. There are internal changes which may affect carcinogenicity.

DR. DUPRE: Excuse me, Mr. Hardy.

MR. HARDY: Yes.

MR. HARDY: Q. Moving to a different study, Mr. Peto, you discussed yesterday in detail an equation, a model, which you developed for predicting the incidence pattern of mesotheliomas, which found the incidence to be proportional to the time since first exposure, cubed.

THE WITNESS: A. Yes.

Q. I would just like to understand a few more things about that model and what sort of process you went through in arriving at that model.

First, I think you, yourself, state on page five of tab nine, which is the paper where that model is most thoroughly discussed, that though you use a power of three point two, that any value between two point five and four would provide an adequate fit to the data that you were looking at?

A. Yes.

Q. I gather by that that there is nothing fixed or specific about the three point two figure necessarily being the right figure for predicting incidence patterns?

A. No. I point out in that paper, it doesn't make much difference though...I mean it doesn't really make...it makes a difference by a factor of less than two for predictions that you make, because in fact you don't start at age nought and

A. (contd.) project the line up. What you do is you observe the incidence thirty years after first exposure, and then project the line forwards and backwards from that point.

So in fact the lifelong risks that results from powers of time don't in fact vary as much as you would expect them to intuitively, and in fact I think on that page...yes, it points out that if you change the estimate from three point two to four, the predicted lifelong risk for men first exposed at age twenty, under the conditions of the insulation workers, would go from fifteen percent to nineteen percent. So the change in predicted risk is really rather marginal, even for quite gross changes in the exponent.

That's explained on page five of tab nine. So for practical purposes it doesn't much matter. I was more interested in the theoretical issues for this. You may know that the particular exponent of time has implications for carcinogenisis, but for the practical purpose of predictings risks it really makes very little difference.

Q. So I gather in coming to this model you started, as you indicated yesterday, with the background, knowing that for some other sets of data, smoking in particular, power functions have been found to be good ways of expressing the data?

A. Yes.

Q. Expressing what the data was?

A. Yes.

Q. Thus it was that that led you to focus in on this particular time to a power...

A. Yes.

Q. determination?

A. Yes. And to investigate the age independence as well. I mean, to assume that...I mean the age independence in a sense is from..I mean from a theoretical point of view is

A. (cont'd.) the most interesting aspect of these data.

Q. I gather that the model did not particularly well fit the Selikoff data in terms of both either early or late exposures?

A. Yes, that's right.

Q. Thus you focused your attention only on that portion of the cohort first exposed between 1922 and 1946?

A. Yes.

Q. Which I think is shown in table two?

A. Yes, that's correct.

Q. Did you, at that point when you found that some parts of the data did not fit the model very well, make any attempts to determine other models that might better fit all of the data?

A. Well, I mean they are actually internally inconsistent in the sense that if you take people first exposed at particular times, and if you look at table two of tab nine, if you take people first exposed at a particular time and have been followed up for a certain period of time, you find that the earlier and later data are actually inconsistent. So, I mean, you know from the first of the graphs in that paper, are based on all the data...figure one, rather, is based on all the data where you are looking at the age independence. There is no exclusion there.

So you know the data is completely irrelevant. So the only thing that determines the risk is going to be time since first exposure, and presumably whatever level of exposure people suffered will, one would assume a priori that exposure patterns wouldn't remain constant over a period of fifty years. It would be rather remarkable if American insulators were subject to identical conditions over a period of fifty years.

So in a sense it is surprising that the data seem

A. (cont'd.) to be more or less uniform over a period as long as 1922 to 1946.

But, just in the purely statistical sense, the data show internally that the risks at particular points in time are substantially lower for people exposed...particularly people exposed since 1946.

Q. So what you are saying is that you are surprised that the data fit your model as well, even for that period?

A. Over such a long period, yes.

Q. Let me ask...the previous table, table one, you show the incidence per thousand men per year for the Selikoff cohort, down in the bottom line, at various years since first exposure, beginning at fifteen and up through fifty-plus.

A. Yes.

Q. It strikes me in looking at that string of numbers that it is possible to interpret the incidence in the later years, say from thirty-five-plus to fifty-plus, to show an incidence pattern where incidence is levelling off, with the exception of the data point for forty-five-plus years, which appears to be anomalously high. Is that an unfair reading of that data? It seems to be in contrast with your conclusion that the incidence rate will continue to go up...

A. It's difficult to discuss this without... I mean, you can't discuss a question like this in isolation. The fact is that there is really overwhelming scientific evidence both from very well controlled animal experiments and in particular on very, very careful studies on cigarette smokers, and a number of ordinary...I mean in the sense that the carcinogen hasn't been identified...all the mesotheliam tumors, which show that cancer rates simply rise uniformly with age, and

A. (cont'd.) usually as a power of time. So against that background...I mean that's very much the sort of normal hypothesis of cancer epidemiology. You expect to see data which go up in that pattern.

Then you also expect to see variations which are internally demonstrable and consistent, in relation to the period of first exposure. It would be surprising if conditions didn't change over time.

Then finally, you take a central period which in fact contains most of the cases...I mean, if you look at the... I mean the actual number of cases that is excluded isn't large. There's a total of a hundred and eighty cases arising in that period when the pattern is consistent, and another fifty-five that have been excluded, that occurred in people exposed before or after that time.

So it seems to me, you know, extremely unlikely that the rates will eventually fall. The analogy between these data and the lung cancer data in smokers is also extraordinarily close. I mean, they are the only two cancers for which you've got an identified carcinogen, detailed data on age at first exposure and age. They follow absolutely identical patterns. I mean, that's a persuasive, if not conclusive, argument that this is the right answer, I think.

Q. I guess what I'm asking is, you tell me absolutely identical patterns, and when I look at that bottom line on table one it just doesn't strike me that the pattern there is the pattern described by your model...

A. There is a graph I want to show...

Q. ...in that the incidence per thousand per man year from 1935 to 19...through thirty-five years plus, from thirty-five years plus through fifty years plus, does not seem to vary all that much.

A. If I can find it....I mean, these are the

A. (cont'd.) lung cancer rates among British men in successive periods from 1936 to 1940, up to 1968, and in fact later data continue to evolve in this way. As you can see, there are extraordinary differences.

The remarkable thing about lung cancer at that time was that it was the cancer of the, the epithelial cancers, which showed this extraordinary peak more markedly than any other. There were others which don't rise in these beautiful straight lines, but none of them showed this extraordinary peak of incidence going up and then falling.

In fact, it turns out that the reason for this is that people started smoking abruptly at the time of the first war, and so you weren't comparing like with like. When you took people who were born in 1900, for example, which is more or less that line there, then they had smoked all their lives. I mean people born in 1900, in terms of the first war they all started to smoke and there's this uniformly, steadily increasing cancer rate.

Whereas if you looked at cancer rates in 1950 to 1955, for example, the rates for people aged eighty in 1950 were born in 1870, so they were aged fifty when people started smoking and they either didn't smoke at all, or they had only smoked in the latter part of their lives, and whereas duration of smoking completely determines the risk, they had minute lung cancer rates far lower than young people did in the same period.

So this sort of completely absurd cancer rate, which is scientifically meaningless, is characteristic if we go to the line for possible exposure.

Now, the funny thing about lung cancer is it's gone from being the cancer with the worst data, in the sense of asbestos, to being one of the best. The graph that I showed you before, which is an analysis...I mean, either of these lines

THE WITNESS: (cont'd.) when you plot it against age or against time since...duration of cigarette smoking, you get an absolutely, perfectly log linear relationship. It's rising absolutely perfectly as the fourth power of time, which is duration of smoking, or the seventh power of age.

So this emphasizes that it is absolutely crucial to allow for the fact that different people, in a given period of time, have different exposures, and when you do a study in 1970, which is essentially what Selikoff is doing, it would be absurd to take it for granted that insulators who started exposure in 1915 had exactly the same experience as those who started in 1930 or 1950.

I mean, the change in the pattern is absolutely gross.

MR. HARDY: Q. As I understand it, it is possible to do those graphs only when information was collected on intensity of smoking, is that true?

THE WITNESS: A. Sorry?

Q. You just showed these graphs that Doll prepared.

A. Yes.

Q. Is it true that part of the data that went into being able to prepare those graphs, information was collected on amount of smoking for the people involved there?

A. Yes.

Q. Having been able to collect that information, he was able to draw those graphs. With the Selikoff data, on the other hand, we don't have that sort of information on amount of exposure?

A. No.

Q. Therefore, is it fair to say that we can't be sure of the relationships, the accuracy of the modelling, in the same way that we can with respect to Doll's findings on smoking?

A. Well, I mean, as I say, from 1922 to 1946 is a fairly long period, which incorporates the majority of exposure. I mean, I don't know much about the history of it, but I would assume that...I mean asbestos exposure was actually pretty low in the early twenties, and I'm not sure that insulation contained a great deal of asbestos before 1915 or 1920, so there may in fact be historical reasons for assuming there was a change in that time.

But the actual...I mean the increase in rates is really fairly substantial. I mean the data we have at the moment in most studies is around about thirty years after first exposure, and the rates in Selikoff's data thirty years after first exposure are of the order of three per thousand per annum, which is more or less what we have observed in the Rochdale cohort, for example, and by the time you get to fifty years after first exposure, they are more than three times as high as that, and very, very significantly different.

I mean, the ninety-five percent confidence intervals in figure two don't even overlap for the period sort of forty-seven to fifty years compared with twenty-seven and a half to thirty.

To look at those data with those confidence intervals and suggest...to suggest that they are not increasing would be absurd, but to suggest that they are going to stop increasing beyond fifty years would be, to say the least, optimistic. I don't think that...I don't understand what you are suggesting. Am I suggesting that one should...

Q. What I'm trying to get a feel for is how well your model fits, and how much data...

A. Well, it fits perfectly in a statistical sense, and it fits perfectly scientifically. I mean I don't know how else to answer your question. It fits in with everything that is known about carcinogenisis in a rather elegant way.

I mean, statistically if fits perfectly, and

A. (cont'd.) it incidentally is consistent, although the numbers are small, with every other study that I could find which has data on mesothelioma rates.

I mean, I think these are reasonably...I'm not sure what criteria you want to apply to this sort of model fitting, but I think it satisfies them reasonably.

I mean, if you think it would be reasonable to assume that the mesothelioma rate is going to fall in the future, on the basis of those demonstrably unsatisfactory data that you get if you take the whole cohort, then I would disagree with you.

Q. I'm not trying to argue. I'm just trying to understand the extent to which other models might, as adequately or more adequately, fit the data that you have used.

A. Well, I mean...

Q. It's a little hard to do in the abstract...

A. You've got in that table which compares the incidence rates at a given time after first exposure, for people first exposed at different periods, you see significant differences in the rates in people exposed first at different times.

So the model that you would have to fit would obviously have to take into account the years in which people were first exposed.

Well, it's difficult to see how you would take that into account, other than by assuming that they had different exposures. That would be the natural way of taking it into account, which I have implicitly done.

I mean, I could do it. The data in the earlier and the later periods fit the same distribution, assuming that the exposures were lower, and so in fact...beyond that I can hardly conceive of a model that could be fitted. I mean I don't...I can't imagine what other model you could fit.

You aren't supposed to answer questions, but what model would you suggest?

Q. Well, I guess one question I have that I'm not sure what the answer would be, but is it possible that the factor which is cubed should include some information on dose? You cube a time factor, but the information on dose is included in your K, which is not cubed, and I just wonder whether another model which cubes dose might...or squares dose, or whatever...

A. That's a separate issue.

Q. ...might be better fitted to the data?

A. No, no. It wouldn't solve this problem at all because you have no information on dose to put into it, so if you find that the rate is half as low in people first exposed in 1915 as those exposed in 1930, then you can either say that they suffered half the dose and that explains it, or you can say they suffered one or two the dose, and square it, and that would explain it.

But that doesn't really solve the problem. You can't establish dose linearity in any of these situations, because the data aren't good enough.

For what it's worth, I mean what few studies there are where duration of exposure has been looked at, they do suggest a linear dose-response relationship in the sense that within a range of fairly short periods the incidence seems to be more or less proportional to duration of exposure, which I think is fairly strong evidence that the effect is linear, but you can't really say more than that.

Q. I think I'll move on to another subject at this point, before we thoroughly confuse each other.

I would like to ask a few questions about the Rochdale study that you worked on, and as I understand it, you, yourself, have never seen the measurement exposure, raw data from the Rochdale plant?

A. That's true, except for the data that are based on those figures in that most recent Lyon publication.

A. (cont'd.) I have those on an individual basis, but none of the previous data, no.

Q. Similarly, the conversion data which was computed from measurements in the plant in the late fifties and early sixties, your review of that data has been limited to the published table of those?

A. Yes. I have actually...I mean, I have seen some of the conversion measurements and the relationship was in fact quite good, but I have never had access to them. They have never been published, or I haven't got copies of them.

Q. But I gather from everything you have said in the last day and a half that exposure information in the epidemiology studies we're reviewing here it is crucial and very important to the question of risk assessment?

A. Yes.

Q. Then I gather you also said yesterday that you believe the Rochdale data is very important data in that effort, and I'm just wondering whether before anybody relies too exclusively on the Rochdale data would not it be very important that that exposure data be given as much scrutiny as any other study that people decide to rely on?

A. Well, as I say, the actual correlation between the measurements in particles in 1960, and the measures in 1961, which is reproduced in one of the tables...I forget in which one of those papers...is actually better than the...is better than those data. I mean, they are not too impressive, but they are really not as bad as the data comparing particles per cubic foot and fibers in the Canadian mines and mills.

As I say, these are data taken in 1960 and 1961, in more or less the same area, without any attempt to establish similarity of conditions. So it's remarkable that they are correlated at all, really, whereas in the Quebec mine and mill analyses, adjacent sampling was done and the correlation was virtually zero.

A. (cont'd.) So I mean, I think in a sense those data themselves, which were first reproduced, I think, in Richard Doll's 1968 paper where he tabulated measurements in 1951 through the early sixties, by the two methods, show that the relationship between what, the first and the second sets of measurements is certainly very much better than what work has been done in attempting to correlate particles and fibers.

Q. I would like to ask a couple of questions about that. First of all, do you know whether this is all of the data available on the issue of conversions, the Rochdale plant, or is merely a subset that happens to have been published?

A. Well, I'm sure it's a subset which happens to have been published, and they have had the most enormous monitoring procedure going on there since the early fifties. They've got, you know, thousands of samples.

Q. Whereas I understand the table from Gibbs' chart, which we have seen here, represents all eighty-seven pair measurements he made in that study, so I'm just wondering whether this subset happens to correlate better than all the data, and I don't know whether it does or it doesn't. I gather you don't know either?

A. Yes, that's true. As I say, I mean, it isn't satisfactory to say such a thing in this context, but I have seen a graph of parallel measurements which were taken, and the correspondence was very much closer than that.

I think the reason for this may be not so much to do with the measuring technique as the fact that a higher proportion of the particles in a situation like the Rochdale factory are in fact fibers. I mean, the great majority of the particles that are measured in mining and milling aren't asbestos at all. Of if they are, they aren't respirable fibers.

I mean, you are measuring the wrong thing, from a point of view of estimating carcinogenisis...carcinogenic potency.

Q. I guess I want to ask you the same question about the Gibbs conversion and something about how that data might be used.

As I understand it, in the McDonald study individual exposure values were attributed to each man in the study?

A. Yes.

Q. Those were based on what job he worked on, in what year?

A. Yes.

Q. Those were calculated from a wide range of particle measurements made over the years in those mines?

A. Yes.

Q. Now, as I understand it, in the work that Gibbs started to do in 1974, he found it difficult to correlate on the basis of one conversion factor, fibers to particles. There was a great range?

A. Yes.

Q. But on the other hand, it's quite possible that at a given site in a mine, the particle...the particle-to-fiber conversion factor is fairly uniform?

A. I think it's unlikely in view of the extraordinary variation...I mean between, you know, between mines and...I don't know if I should try doing it with the paper now...but there's a table in there which lists different mines in different sites. I mean, even within broadly similar activities, there's extraordinary variation.

As I say, I mean I think the impinger is measuring the wrong thing. It isn't...yes. I mean, the values for fiber screening vary from, the ratios vary from three point five to forty-seven point four, in bagging they ranged from three point eight to thirty-one point one...

Q. In different mines?

A. In storage they vary...yes...in storage they vary from nought point eight to twenty-two point eight.

So, I mean, it's difficult to see that they are of much value if you are trying to do anything other than...I mean, I would doubt whether they would be much, of much value for controlling bagging in that particular mine.

I mean, secular changes aren't analyzed, but in terms of generalization they are evidently not of any value at all.

Q. But the possibility does remain though, that the particles-to-fiber conversions in particular locations may well correlate well? Those from mine to mine, in what are apparently similar operations, may not correlate that well?

A. Well, that's conceivable, but unlikely. If your criticism of the Rochdale data is in fact that although there is a fairly good correlation in these rather uncontrolled measurements, which were taken a year apart, that in fact they may be completely uncorrelated because there were lots of rotten data that they didn't publish. Then your interpretation of the Canadian data is that although the correlations are so-called, they are ludicrous, even within mines and within comparable areas between mines, but your interpretation of it is in fact if we had done the same measurement in the same area in the same mine, we would have attained wonderful consistency.

Your position isn't logically untenable, but it seems to be implausible.

Q. I'm just trying to understand what we know about Rochdale and what we know about mining data. That's why I'm asking you about these things.

I won't be all that much longer, but it might be a half hour.

DR. DUPRE: All right, shall we...

MR. HARDY: Counsel suggested you might want to break for lunch.

MR. LASKIN: Mr. Peto, just before you take that away, I just want to identify that slide. Are you plotting the figures that are in your tab one at table five?

THE WITNESS: Yes. 1960 against 1961.

DR. DUPRE: Shall we rise, then, until 2:15. I hope the parties will understand that Dr. Mustard's absence this afternoon is occasioned by his obligation to attend funeral services.

MR. LASKIN: Just before...because I'm not sure if everyone...I'm not sure that Linda Jolley may have another commitment as well...but I just want to go back to the schedule for August that we laid out, and there is one date that is committed that you didn't mention, Mr. Chairman, and that's Monday, August 24th, which is tentatively right now committed to the Toronto Occupational Resource Center.

DR. UFFEN: I have no notice of that here.

MR. LASKIN: Well, I may have to discuss that with you later, then.

THE INQUIRY RECESSED

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THE INQUIRY RESUMED

DR. DUPRE: Ready, counsel?

MR. LASKIN: I'm sorry, Mr. Chairman. We are ready to go.

DR. DUPRE: All right. Just before we return to the witness, the Commission reconstructed the dust levels that surrounded its...yesterday's caucus on the days it was setting aside, and the amendments, which indeed run true to the record, were to be as follows: Monday, August 24th, afternoon only; August 25th, all day; August 26th, afternoon only, and

DR. DUPRE: (cont'd.) then the rest as before.

M. CASGRAIN: The 25th is all day?

DR. DUPRE: The 25th is being held all day. The 24th and 26th are afternoon only.

Ready, Mr. Hardy?

MR. HARDY: Yes, sir.

DR. DUPRE: Please, proceed, sir.

MR. HARDY: Q. Mr. Peto, we had a good deal of discussion yesterday about this relative risk of five, which is still on the board, which is derived from the lung cancer results for the cohort at Rochdale, post 1950. As I understand it, the data from which that relative risk was calculated was presented by you for the first time in your presentation in Lyon, which is tab seven?

THE WITNESS: A. Yes, that's right.

Q. I believe when you presented those results in Lyon, and in your discussion of them, you indicated that this result and this higher relative risk than in the older cohort at Rochdale was largely due to chance?

A. Well, as I said this morning, that's one of the possible factors that could have produced it, yes.

Q. Actually in the Lyon paper you said, it seems likely that the increase is largely due to chance?

A. Yes.

Q. Then I gather, given that fact in your discussion in the paper, you go on to suggest that the eventual relative risk for lung cancer among these men in the post-1950 group is probably between two and three?

A. Yes.

Q. Which would be considerably lower than the relative risk of five which the current results show?

A. Yes.

Q. Then you go on further to indicate that in those terms, the relative risk found up to date in the post-1950 cohort is not all that different from the relative risk previously found, of two, for the pre-1950 cohort?

A. Well, I mean they differ significantly. What I said in the discussion of that paper is that I find it difficult to explain the difference, and so I assumed that various factors, noteably chance, had produced this difference, and it was likely that the relative risk was in fact higher than the one that we had actually observed in the earlier cohort, and probably lower than the one we observed in the later one.

That I was in fact essentially amalgamating them and saying that the difference seemed artifactual, and it seemed likely that by chance the early estimate was too low and the later estimate was too high.

Q. Right. And actually don't you indicate that the probable two to three relative risk which you indicate is likely to exist for the post-1951 group is, as you put it, in reasonably close agreement with your early analysis in 1978, which was based on the assumption that the relative risk would be about two, in men who had suffered cumulative exposures of about two hundred fiber milliliter years?

A. Yes, that's right.

Q. As I understand it, after you discovered these eight lung cancers in the post-1951 group, you went back to the records and determined that all eight of them were cigarette smokers?

A. Yes.

Q. Which means that there was a higher proportion of cigarette smokers in this group than in the plant as a whole?

A. Oh, I mean virtually all lung cancers among asbestos workers who are cigarette smokers, as they are in the general population. I mean, in the slide that I showed you yesterday...

A. (cont'd.) I forget the exact numbers, but I mean there was a total of five nonsmokers in the entire cohort, that Professor Selikoff studied, compared with nought point nine expected at nonsmokers' lung cancer rates. I can't remember the figures on which he had detailed information, but something of the order of three hundred cigarette smokers.

I mean, it's normal for something of the order of one in a hundred lung cancers, either in asbestos workers or in the general population, to be nonsmokers. So it would be extraordinary if we had observed a lung cancer in a nonsmoker in this cohort.

From the point of view of compensation, of course, that raises an interesting question, that there is no particular reason to have different compensation rules for smokers and nonsmokers, because in either case the probability that the lung cancer was caused by asbestos is the same...given that the relative risk is the same.

Q. But in terms of the actual numbers of lung cancers occurring among people who worked in asbestos plants, the great majority are going to be among smokers?

A. Yes. Yes, virtually all.

Q. Further in your discussion in Lyon, you go on to discuss some of the difficulties in that the data demonstrates exists in discriminating risk levels based on static measurements in the past, which is what we have in most of the studies. And you suggest that personal sampling, which might reveal very high transient exposures during certain activities, may inordinately contribute to risk.

A. Yes. That's pure speculation. I mean, I just thought it was a question worth investigating, which hadn't been investigated. I mean, I have no information either way on that.

Q. I gather you must be suggesting it for some reason. Is that because biologically it might be a predictable...

A. Well, no. It's just a question which is obviously important in terms of relating risk to measured levels, which hasn't been examined properly. I think the data probably exist that were used to examine it. I mean, various situations where monitoring has been done over the last ten or fifteen years, and I suppose particularly personal monitoring.

It would be interesting to see whether there were occasional hours or days when anomalously high levels were recorded, and see whether or not they could, averaging out, account for a substantial fraction of the total amount that they might have breathed in.

Q. It could be possible then that if that is the case that there may have been more of these sorts of high transient exposures in some of the historical cohorts, accounting for a lot of the disease which, under current control conditions in factories, tend not to occur very often?

A. I don't really know about that. I mean, I have no idea if they are more or less common now than they were then.

But as I say, there is no evidence to suggest that that's the case. I simply thought it was something that might be worth looking into.

Q. You suggest then that...in the Lyon paper... that if in fact it is the case that high transient exposures were inordinately important, imposition of work practice requirements that are specifically aimed at prevention of those sorts of transient exposures may very well turn out to be more useful than overall time-weighted average means of controlling exposures in plants?

A. Yes, if those practices weren't included in the time-weighted average measurements, which they might tend not to be. I mean, this really is a subject that I'm not an expert on, but I mean one hears anecdotes that people

A. (cont'd.) take off their personal samplers to do certain things, and so on and so forth. I mean, I'm not at all sure even now how good the data on what actually goes into peoples' lungs are.

Q. But I guess it is clear from your suggestion of these ideas in the Lyon paper, you do think they are areas worth exploration?

A. Yes, and as I say, I suspect that data that would enable one to at least have a preliminary look at them are already available. They are just not being looked at.

Q. I would like to talk in general about some of the...or ask you some questions about the elements that you believe should be considered in constructing risk assessments, and I gather from the discussion we've had over the past day and a half that in predicting risks at various levels from asbestos exposure, talking about risks of mortality, that you would not consider asbestosis to be a very important factor under current conditions and the sorts of exposures that occur?

A. It's not that I don't think it's important. It's that it is of the same order as the risk due to cancer...due to lung cancer in particular. If you were legislating for people first exposed when they were sort of sixteen or twenty, the mesothelioma risk and the lung cancer risk I think would be of the same order. The reason that mesothelioma accounts for a smaller proportion in most cohorts is because you are averaging out people who were exposed when they were young and people who were exposed when they were old, who have a much lower risk of mesothelioma, of course.

Asbestosis is of the same sort of order. In fact, usually it accounts for rather less excess mortality than lung cancer, and the difficulties of estimating previous exposures are so enormous that really it seems to me to be not worth complicating the issue with a lot of arguments about a disease for which one

A. (cont'd.) really has no good prior grounds for assuming certain forms of dose-response relationships. In fact as I say, I take issue with the Simpson Report because it failed to consider mesothelioma, which particularly for nonsmokers, is the major risk, major cancer risk.

But their approach to basically examine cancer risk and then to double it in case asbestosis turns out to be of the same order, I think is quite sensible. Because I think that it's a very...it's a speculative waste of time, I think, to try and argue about whether there will or won't be a threshold for asbestosis. I mean, it could be argued on the basis that there isn't, and it could be argued plausibly, biologically that there is. The errors in the dust, earlier dust measurements, as I say, are so large that a factor of two is really rather beside the point.

So it's not that I don't think it matters. It's just that I think it's rather pointless to get too bogged down in a debate about it.

DR. UFFEN: Would it interrupt you if I asked a question on the same topic?

MR. HARDY: No, certainly. Go right ahead.

DR. UFFEN: It's in the nature of a tidying up a little bit.

If we refer to tab three, your other paper that we've looked at a great deal, on page 487 in the left column is the section where you had been talking about these old dust measurements and pointed out how crucial it was. It says at page...I think it's the last sentence: "However, this

> crucial assumption is open to dispute and cannot be proved until further followup has provided a more accurate estimate of excess mortality in more recent employees."

DR. UFFEN: (cont'd.) Are you able to say whether that can now be done, or if not, how long might it be before it could be done?

The further followup is what I am referring to.

THIS WITNESS: This is the further followup. I mean, these data, of course, weren't available when I wrote this paper.

DR. UFFEN: So what you are referring to here ...

THE WITNESS: I mean the further followup is suggested...

DR. UFFEN: ...has now been done?

THE WITNESS: Yes, and it shows that the risk was significantly higher during the further followup. That's the anomaly.

DR. UFFEN: Would you say then that it's no longer open to dispute? That you have demonstrated..

THE WITNESS: Well I must say, I mean I have always felt that excess cancer rates were much better measures of exposure than anything else. I mean, if you want to estimate what the cigarette consumption of various age groups in Britain was over the last fifty years, the national lung cancer rates give you a very much better idea than the rather poor surveys which have been done at various times.

It's true. I say that quite seriously, and I think the same is true in relation to the effects of asbestos, that in fact you can get a much better idea...at least relatively, if not absolutely, of what exposure conditions were by looking at mesothelioma and excess lung cancer rates than by getting dust measurements and then arguing about whether you should multiply or divide them by ten.

MR. HARDY: Q. That would assume a prior knowledge of the dose-response relationship, though, wouldn't it?

THE WITNESS: A. That it's linear, yes. But, I mean, all you have to do is assume that they are

A. (cont'd.) monotonic to decide that the exposure was higher, rather than lower, or if the risk is higher rather than lower.

Q. And do you do it with relative past exposures, not absolute past exposures?

A. Yes.

Q. Now it also assumed that the risk does not vary from type of plant to type of plant?

A. No. I mean I'm talking about level of exposure.

Q. To the relevant biological substance?

A. In the sense of being the sort of multiplying factor in the incidence equation, but in this particular case there wasn't a large difference in the sort of activity that took place. So in this case it would be in some way related to dust level.

As I say, I mean if you want to compare crocidolite with chrysotile, I mean there were absolutely no data on the effects of measured levels of crocidolite, and nobody has any idea whether fiber-for-fiber it's very special or the same.

Q. So I gather then in doing a risk assessment you would place most of your reliance on predictions from mesothelioma and lung cancer?

A. Yes.

Q. I gather you believe both..first of all let's take mesothelioma as clearly a dose-related phenomenon.

A. Yes, in the sense that...only rather crudely. I mean, the risk is higher in people who are exposed for longer, and it's higher in people who anecdotally have heavier exposures, but I mean, as I say, that's the sort of level at which the dose response has been demonstrated...and the same is true for lung cancer, of course, the same thing.

Q. I guess...maybe you answered this yesterday

Q. (cont'd.) but I would sort of like to ask you again...in terms of setting standards, which may well be premised in part on an assumption about future risks, do you believe it useful to distinguish among different types of plants, given differing results in the historical studies, from different types of plants?

A. Yes. I think so. I don't know what the situation is in the chrysotile mines, for example, in Canada. I mean, if it turns out that the fibers that were being counted when the comparisons were made were predominantly short with particularly...they were predominantly thick, then it might be interesting to look at the data again in terms of fibers of comparable size, because the range which is regarded...the range that is included in the legal definition of a fiber, in terms of the two fiber standard, is actually quite wide. That may be the explanation for the anomalous relationships.

But otherwise, I mean it would be difficult to... yes...it would obviously be unreasonable to impose the same standards in a situation where the risks, you know, according to the measures on which you are legislating, were very much lower. It would seem illogical.

As I say, I mean that's really one of my main disagreements with the Simpson Report. I mean, it didn't really address this issue at all. I mean, it used the data on Canadian mines, which I think are entirely irrelevant to the situation in manufacturing anywhere, let alone in England.

Q. But perhaps very relevant to a situation in Canadian mines?

A. Oh, evidently, yes. But I would like to see the anomaly resolved. As I say, I have never seen good data on the distribution of the fibers that have been counted in the Canadian mines, and in particular I would like to see gross averages, and I would like to see sort of gross average exposures

A. (cont'd.) of the people exposed for more than twenty years at sort of medium-high to high levels...in whom there is a two or threefold relative risk for lung cancer. I mean, although the overall excess is small, I mean within that cohort there are groups who suffered substantial excesses of lung cancer, and it would be interesting to see just how anomalous the data are when you look at that subgroup who really contain most of the information.

Q. I guess I don't quite understand that last point. What anomaly are you trying to solve?

A. Well, there aren't many cohorts of relatively recent asbestos workers in which the relative risk for lung cancer is much more than two or three. That's a sort of typical figure. I mean, the relative risk of ten have tended to be in the very heavily exposed older group, and within the Canadian mines there are individuals...I mean the people who contribute the top points on the dose-response curves...who have relative risks of three or four or five for lung cancer, and in fact they are the people who have worked there for a long time under heavy exposure conditions.

It would be interesting to isolate those cohorts, to analyze them to look at data on those subgroups to compare their lung cancer rates with their mesothelioma rates, and in particular to try and get some idea of the sort of distribution of fiber sizes that they were thought to have been exposed to, because as I said, I know nothing about this, but I assume that mining and milling even have different characteristics of fiber distribution associated with them - fiber size distribution, and this seems to me not to have been gone into in much detail.

Q. One last thing I guess that I would like to talk to you about is the risks you calculate for schools in the model that you proposed in tab ten, the calculated risks in schools.

Q. (cont'd.) First of all, is it true that the approach risk assessment that you go through here is the same approach that you would have proposed for occupational exposures?

A. It's exactly the same yes. The only reason I submitted it, really, is because it's the only time that I have gone through in some detail the sort of arithmetic that is being done, but in fact it is exactly what I actually did for that Lancet paper in 1978. I mean, it's exactly what I was recommending in that sort of more general New York Academy of Sciences paper in 1979.

Q. Although in the last paper...

A. I mean, the methodology is exactly the same. The only difference is that now there are actually some data to back up the models that I suggested. Four years ago it was rather hypothetical, but I think that both the lung cancer and the mesothelioma data in fact support those models now, and I think they are more clearly appropriate than they were then.

Q. I gather then in calculating the numbers of predicted deaths at various exposure levels from this model, you used the data from Selikoff's insulation workers?

A. Crudely, yes, although in fact in that schools report...which I can't find at the moment...I didn't use the same population figures for lung cancer as I had done for mesothelioma, because for lung cancer you have to get specific rates for smokers, and so actually I used data from the American Cancer Society's 1966 publication.

Q. The risk you are predicting here for lung cancer is for smokers?

A. I did both, actually. I think there is a calculation for smokers and nonsmokers, isn't there?

Q. I think there may be calculations. I'm not sure you provided a table for nonsmokers.

A. All right. Perhaps I didn't provide the table. The calculation is identical. You just put different lung cancer rates there, and say the lung cancer rates are sort of, you know, a tenth or a twentieth as much, so the predicted risks will account for a twentieth as much.

Q. One question I had was in finding a mesothelioma risk from the Selikoff data you were obviously talking about a cohort where peritoneal mesotheliomas were found? Correct?

A. Sorry, which prediction of risk?

Q. In order to predict risk in this paper, you extrapolate from the risk found in the Selikoff data for the insulation workers, correct?

A. Yes.

Q. Those workers had a substantial known peritoneal mesothelioma ?

A. Yes.

Q. Which you attributed to amphiboles?

A. Yes.

Q. I guess the question I have is, will school children also be exposed to amphiboles, or if not, may they just be exposed to chrysotile, in which case shouldn't that peritoneal risk be excluded from this calculation?

A. Probably, yes. As I say, the purpose of this calculation really wasn't to provide definitive answers. It was to illustrate the methodology.

Q. That's right.

A. Yes.

DR. DUPRE: I was just wondering if you could explain why you agreed with the premise of that question, which would be to the effect that there are no amphibole fibers that can find their way into schools, it was all chrysotile?

THE WITNESS: I don't know what sort of fibers have been found. I mean, is it in fact the case that they are all chrysotile?

DR. DUPRE: Well, as I...maybe I just wasn't following your dialogue, but if you were going to take account of fiber type in terms of peritoneal, the risk of peritoneal mesothelioma in school children, there would be a difference given your own finding on peritoneal mesothelioma...if there was in fact only chrysotile in the schools, wouldn't there?

THE WITNESS: Yes, that's right. Although of course the funny thing is that although the risk of peritoneal mesothelioma seems to be lower in chrysotile workers, the risk of pleural mesothelioma, if anything, seems to be higher.

As I said, the absolute incidence of pleural mesothelioma, as I said before, is actually higher in our cohort, for example, than it was among American insulation workers who had fairly heavy, prolonged exposure.

So it's difficult to know. I mean, they are necessarily order of magnitude calculations. As I say, I mean that's another factor of two, and in a sense I don't think it's worth getting too bogged down in a factor of two in these calculations. The error is larger than that.

MR. HARDY: Q. I gather that what you did in tables five and six...no, tables four and five...in this paper then, is using your model and using the Selikoff data as a means of giving you a dose response, you calculated the number of deaths that your model would predict per ten thousand persons exposed for six years at one fiber per milliliter?

THE WITNESS: A. Deaths per hundred thousand, yes.

Q. Deaths per hundred thousand?

A. Yes.

Q. All right. In doing that, as I understand,

Q. (cont'd.) you are certainly not predicting those for schools because exposures of one fiber per c.c. just are not at all common in schools?

A. No, no. I mean, I doubt if there are any schools where the average levels were a hundredth of a fiber. As I said, I would expect that the risks would be a hundredth of that, or less. But I mean, I'm guessing. I haven't actually seen good data on asbestos figures in schools.

Q. Well, you do use one thing here on page fourteen, where you calculate the risk of excess lung cancer in smokers, and you talk about if the concentration was point zero zero two fibers per milliliter. Is there any particular reason you chose that figure?

A. No. It's better to choose zero zero two than zero zero one when you are doing a calculation, because you can tell the difference between two and one when you are trying to explain how you did the arithmetic.

I would guess...but I mean this is a pure guess... that that is the sort of...that's the order of average level in those sort of circumstances.

But as I say, the purpose of this paper was to illustrate how the calculation should be done, and I don't know if I emphasized it in this report, but I mean when I corresponded previously with the EPA about this, I emphasize that the most important thing to do is in fact get some idea of what the fiber levels were in schools and it would be silly to take it any further until these data were available.

Q. I gather, and you made the comment yesterday, that EPA's initial risk assessment for schools you found grossly exaggerated and that was one of the things which sparked you to send this information?

A. Yes.

Q. I gather you reach a conclusion from these data that the risk of lung cancer and mesothelioma from the exposures likely to occur in schools is negligible, very small?

A. If the fiber levels are as low as I think they are. But as I say, it depends very much on what the fiber levels in schools are. If there are a few situations where they are substantially higher than that, then the risk could be substantial. But if they are as low as they appear to be, from what anecdotal information I've heard, then that would be so, yes.

Q. I gather from the way you have discussed these sorts of risks that you would cast doubt on any program to rip out ceilings or take extraordinary measures when the exposure levels are of this order of magnitude?

A. I think so, yes. I don't know. I think that the first thing to do would be to look at the fiber levels, and then to discuss the technical question of whether or not they are likely to get worse in the future when friable material deteriorates and so on. But my impression is that there has been an overreaction. But as I say, I think it's difficult to judge until you have better data on what the exposure levels actually were...or are, rather.

Q. I gather to the extent then what you are saying is that the fiber levels are around point zero zero two, the number which you calculate here?

A. I really don't want to offer that as an estimate. I mean, I can't...I have at various times seen estimates of fiber levels in buildings, including schools, and some are published by the Asbestos Information Council in England. I don't have them with me, and my recollection is that they were of that order. But I think that I chose that round number simply to illustrate how the calculations should be performed.

Q. I'm not trying to pin you down. I'm saying if that's the number in the schools...and there must be some schools where that's the number...some may be higher, some may be lower...but in a school with an exposure level like that, what your calculations tell you is that the risk is so minimal that expensive remedial measures probably would not be justified?

A. Yes. I mean, it's a matter of opinion. If you think that the...I mean the death rate is of the order of one per hundred thousand. This isn't a death rate, it's a lifelong risk. I regard that as a negligible risk, but that's not my job to decide that. In America they get very upset about risks of ten to the minus eight when they are legislating on carcinogenisis, which I think is a bit silly.

I don't know what the situation in Canada is, but in America you aren't allowed to cause cancer at all, which leads to sort of legislative difficulties. It kind of admits that there isn't a safe threshold.

Q. Just maybe to put in context the one in a hundred thousand lifetime risk, maybe we could try and do the same calculation as yesterday. Yesterday you talked about what the average shortening of life expectancy would be at a one percent risk, and I think you said it was about two months?

A. Yes.

Q. Can you calculate what the average shortening of life would be for a one in a hundred thousand risk?

A. Yes. It's a thousandth of a month. It's about an hour, isn't it?

MR. HARDY: I have no further questions.

DR. DUPRE: Dr. Uffen?

DR. UFFEN: As a matter of fact, this last little bit covered the questions that I might have in mind.

I have one which I think may be in the category of not a bright question, but it will clarify things for me

DR. UFFEN: (cont'd.) I was very interested in your model, and Finkelstein's, and you start out with the assumption that the incidence is proportional to some power of time - I equals T to the K.

THE WITNESS: Yes.

DR. UFFEN: And astonishingly that data...well, you've showed us this...and astonishingly for us anyway, the value of K lies between three and four, for an extraordinary number of different data.

Is there any information contained in the parameter B? Can anything be extracted from B in your equation?

THE WITNESS: The constant factor.

DR. UFFEN: If it is...I call it parameter because I'm not sure.

THE WITNESS: No. I mean, that's the constant factor which is determined by the level of exposure and the duration of exposure, and I mean as I say, the calculation in table one of the...on page four of the schools report...what tab is that one, sorry? I've forgotten what tab it is.

DR. UFFEN: The school is tab ten.

THE WITNESS: Tab ten. On page four of that there is a table which, you know, I mean suggests the way in which different durations of exposure might be expected to reflect the risk.

So that's one of the factors that contributes to B, and by assumption, if you assume linear dose response, then the other factor is simply the fiber level, whatever that is. But I mean, as we've said several times, there aren't good data to prove that the dose response is linear. But I think you have to assume it because there isn't an alternative assumption to make.

DR. UFFEN: Can I try something on you? I went through this and I developed an expression for that parameter B,

DR. UFFEN: (cont'd.) which turned out to be log I zero over two zero the K, where I zero is the incidence at time T zero at the definition...T zero being the first exposure to asbestos. I zero would be, if there is any, an incidence that was natural in nature.

THE WITNESS: That's the value of B.

DR. UFFEN: Yes.

THE WITNESS: But if the background was zero, which it could conceivably be, the log of zero is minus infinity...

DR. UFFEN: One of our problems, of course, is whether background incidences are zero.

THE WITNESS: No, but the relationship doesn't depend on the background incidence. I mean, the assumption is that the incidence due to asbestos...

DR. UFFEN: I found it did, unless I have made some kind of mistake here. I found that...you are closer to the board, would you write these things on for me? There's not very many of them. Tell me whether I've gone astray.

Or I could go over and do it.

I'm in horror that I've made some stupid error in here and it's going to be demonstrated very quickly.

THE WITNESS: I can tell you...are you talking about the...

DR. UFFEN: I'll write it down on the board what I've got here, and you criticize if for me and tell me whether I'm too slow to learn.

You start out I is proportional to T to the K, another way of writing that I is equal to B to the K. You take the log of I, which would be log B, plus K, log T, which is another way of writing...this is just another constant, plus K log T...that's a T.

Then let I zero be the value of I when T equals

DR. UFFEN: (cont'd.) T nought, and T nought I'll define as the time at which exposure to asbestos dust occurred.

THE WITNESS: The definition of T is the time since exposure began. The definition of T is the time since exposure began, so if it was obvious you were exposed at age twenty, then at age fifty, T is thirty...T is the time since first exposure.

DR. UFFEN: This is very fundamental and I wanted to make sure that that is...T...

THE WITNESS: The definition of T is the time since first exposure to asbestos.

DR. UFFEN: Then what I have done is an alternative hypothesis.

THE WITNESS: You've got to start with an alternative equation then.

DR. UFFEN: No, no, just a minute. I can assume, as you did, that this may be the way the data are, and perform the analysis...and if I find to my pleasant surprise...

THE WITNESS: I'm sorry, what's T defined as? I don't know...what's the definition of T there?

DR. UFFEN: Time.

THE WITNESS: The time since when?

DR. UFFEN: I'm defining that. I don't have to say time since when - time since the beginning. And then I will proceed to put in time since any other event, such as this one. If I continue this, I end up with the expression at the end that's the logarithm of the intensity is equal to the logarithm of I nought over T nought to the K, plus K log T.

That's just an expression like Y of A, plus B X.

When I take this and I start fitting it to the data, I don't see any reason why I wouldn't find that it was precisely the same as yours with respect to K. Instead of having

DR. UFFEN: (cont'd.) a factor B here, I should be able to read off the graph the value of this thing, and where it would represent any incidence that may have existed in nature prior to exposure to asbestos, it might be very, very small.

Now, as I said a few minutes ago, I would like you to criticize it and if there's a great...

THE WITNESS: But I don't understand what T means. I mean, I can't criticize it because I don't know what you mean by T in the equation. What I mean by T is time since first exposure. I mean, T is time since first exposure in the formulation as I have presented it. That's its definition.

But you have to specify what time is. If you are talking about time since the year dot, you can't...I mean if you defined it as being the time since the creation of the universe you aren't going to see a very large change over the thirty year period, because you have twenty thousand million to the power of three and a half, and twenty thousand million and thirty to the power of three and a half. That would have to be checked as the amount of the ratio.

I mean, T has to be stringently defined. In fact very slight differences will grossly change the value of the exponent as well.

As I showed for lung cancer, I mean in fact you can fit lung cancer rates by either saying it equals age to the power of seven in smokers, or that it equals age minus twenty, which is duration of smoking, to the power of four, and in fact these are roughly proportional to each other, so your definition of time is critical, and in turn determines your estimate of the exponent.

I mean, I don't understand what T nought means.

DR. UFFEN: The one that I put in? I just defined T nought as the time at which the group began to be exposed to asbestos. I was trying to allow for the possibility

DR. UFFEN: (cont'd.) that there may have been some incidence that could exist along with it that had nothing whatever to do with asbestos, that came about because of the existing natural environment, dust in the air and the sort of thing that the whole of humanity grows up with.

THE WITNESS: I think that since...the fact that the effect is independent of age, in fact that the mesothelioma is being initiated by asbestos, in this case the background level at least for initiation probably doesn't matter very much.

I mean, for other carcinogens which act at later stages, such as irradiation, and perhaps asbestos in relation to lung cancer, background rate matters very much. So, for example, in smokers who have the high background rate of lung cancer, if you like, their high background rate of lung cancer completely dominates the effect of asbestos.

But for mesothelioma where it seems to be acting as an initiator in isolation, I think the background effect is irrelevant. I think you simply add it to the effect of asbestos.

DR. UFFEN: I'm looking for a possibility of extracting it from the data by your technique. I guess it's not there. Okay. I'll think about it some more.

You've thought about it for several years, I've thought about it for two days.

DR. DUPRE: Just a few questions, if I might, Mr. Peto.

First of all, in your methodology on the school situation, the methodology of your risk assessment in the schools, is, as you say, it's the same as the methodology used for occupational risk assessment.

Do I take it then that your paper assumes that the very young will not be more susceptible to a carcinogen than later age groups?

THE WITNESS: Yes. That's...in fact there were virtually no data on the effects of carcinogens in children, apart from radiation, and there the effect that is well documented is in terms of leukemia, which is an unusual cancer anyway and has an unusual age distribution. There is a possibility that children are peculiarly susceptible because their cellular turnover rates are higher and that in a growing organ you could have very much higher sort of mutational initiation rates...whatever the initiating event is.

It is difficult...I mean in the absence of any data I think it would be unreasonable to put that into the model. You have to bear in mind that the possibility exists, but I don't see how you can take it any further than that.

DR. DUPRE: There is no data from irradiation that you could use?

THE WITNESS: The data on radiation are really rather irrelevant, aren't they? Because it's such a different situation. I mean you haven't...in fact the effect of radiation almost exactly mimics the effect of asbestos in relation to solid tumors. You get exactly the same effect of an increase in relative risk, and it isn't absolutely clear whether the relative risk stays constant or then falls over time.

So, I mean the effect of irradiation, it seems to me that the effects of irradiation and asbestos, in their multiplicative effects with other tumors...in other words, in relation to lung cancer for this purpose...may be the same.

But I mean the major effect in children, as I say, is leukemia, and it's unlikely that asbestos causes leukemia because it doesn't reach the bone marrow quite as easily as radiation does.

There is the other question that I mentioned earlier, which is that...I mean, since asbestos very clearly

THE WITNESS: (cont'd.) initiates the mesothelioma process. It affects the first stage of whatever the process, carcinogenic processes for mesothelioma.

The possibility ought to be borne in mind that it is also capable of initiating the lung cancer process, for which there are no data, as I have said, on people who have been exposed to asbestos and then started to smoke. I mean, the data are all on people who started to smoke and who then were exposed to asbestos. So for that reason also, it's conceivable the effect in relation lung cancer would be greater than it would have been at older ages.

But I would be very reluctant to try to quantify any of these things.

DR. DUPRE: The Simpson Report, as you may recall, made reference to the possible susceptibility of the very young. When Dr. Acheson was here, I asked him what he meant by very young and the answer was zero to ten. Is that what you would think of as the very young when you think of susceptibility of children, say to ...

THE WITNESS: I don't really know what the cellular turnover is in different organs, and if you are talking about leukemia, I would have thought not because I would have thought that the immune system is pretty well differentiated earlier than that, that in fact in terms of development the immune system, which is presumably what determines peculiar susceptibility to leukemia, that that was more or less completed at an earlier age.

I mean, you know, only children get retinal blastoma because their retinas have completely stopped dividing by the time they are five or six. In terms of the epthelial organs where most cancers occur, and particularly the lung...I mean the lung is still growing at age ten...so I suppose that's probably a reasonable range. But I wouldn't like to...I mean it's a difficult question to answer in detail.

DR. DUPRE: Thank you.

Switch to another area. In your paper, tab number two, on page 113, you point out of course the critically important assumption that a dose-response curve is linear.

Then you say at the top of the page, "If dose response is quadratic, the current British standard of two fibers is probably satisfactory."

Now, as I understand it, if I had a dose-response curve that was quadratic, what that curve would show me is that at low levels of dose there is a uniform response, there is a positive response but it is constant right down to a zero level of dose. Isn't that what a quadratic dose-response curve would show?

THE WITNESS: No. It just means that the risk, the excess risk, is proportional to the square of dose. I mean, if you observe data...I mean if the data, the particular data you are analyzing are at dust levels of a hundred, fiber levels of a hundred...sorry, fiber levels of ten fibers per c.c., then if you reduce that by a factor of ten as you go from ten fibers per c.c. to one fiber per c.c., then you will reduce the excess risk by a factor of a hundred.

But as I say, there is no indication that that's the case. I mean, I think in a sense there are theoretical grounds in the case of mesothelioma for thinking it's not, because mesothelioma initiates, and the fact that long or short exposures seem to produce more or less the same pattern, it seems in that case to suggest it isn't doing anything else.

So I think there are theoretical grounds for thinking that the dose response is linear then, and in a sense, there's sort of the opposite argument for lung cancer, that you've got a background process in either smokers or nonsmokers, and you are affecting a different stage, which is later on.

Again, although it's a different stage, it would

THE WITNESS: (cont'd.) look as if a single part of the process is being affected and I would be surprised if the dose response weren't roughly linear.

As I say, what limited data there are support that. So I think that for legislative purposes that has to be...yes, I mean I think it's a red herring. I think you have to assume linearity.

DR. DUPRE: Just one last line simply to help serve my mental review processes. You are one of a most helpful and impressive line of visiting professors who have tried to educate us in our summer school. I try to, sometimes perhaps unfairly categorize some of my various professors in the various schools.

Is it fair to say, Mr. Peto, that when all is said and done you are much closer to, if you are not a part of, what I like to call the fiber dimension school, and are basically rather suspicious of the fiber type?

THE WITNESS: Yes, that's completely true. I mean, I would suspect that the only influence of fiber type is certain long-term chemical changes, that the major differences in both migration and carcinogenic effect are completely determined by fiber size, and that glass fiber of the same dimension would in fact have the same effect...at least in relation to mesothelioma.

DR. DUPRE: Now that leads me to ask you this, thinking of another set of schools. There is one school that would seem to want to tell us that the biological effects of asbestos are overwhelmingly a matter of physics, while another school admits the possibility of chemistry. The chemical school, as I understand it, is basically differentiating among fiber types in that it points out the chrysotile, given its chemical composition, may be less acid resistant than the amphiboles, whereupon this particular school tells me, it is

DR. DUPRE: (cont'd.) possible that chrysotile fibers, given their lower acid resistance, have a higher propensity to dissolve than amphibole fibers.

Now, does what you just said make you a member of what I call this chemical school?

THE WITNESS: I think it's certainly yes. As you know, the evidence on this isn't good. The difficulty is that animal experiments of necessity take place over a fairly short time, and in fact these dissolution effects aren't substantial over periods of a few days or weeks. In human beings you are talking about the effects fifty years later when such effects would obviously be extremely important.

It wouldn't surprise me to find that there were differences in fiber type. As I say, I just don't think that the evidence is good. I mean, it wouldn't surprise me if...there are some anomalies in relation to asbestos altogether which are really not properly understood or discussed. I mean, it's curious...it's very difficult to see a plausible mechanism which will lead to a doubling of lung cancer rates, for example. You can do it mathematically very easily, but it's very difficult to make sense of it biologically. I mean to double the lung cancer rate. In theory, the natural way to double the lung cancer rate is to double the rate of the last change from precancerous cells to cancerous cells, but it's very difficult to see what change in the surface of the, in the bronchial epithelium can take place which is going to double the rate at which that change takes place and is going to continue for twenty or thirty years, or forty years, after exposure has ceased. Yet the data clearly show that that's what's happening.

It's a very mysterious carcinogen.

What laboratory evidence there is in relation to asbestos, I suppose suggests that it acts by breaking chromosomes. I mean, that's its action, cellular action, is to interefere, apparently physically, with DNA, and

THE WITNESS: (cont'd.) that would make sense of the notion that its effects are primarily physical and determined by fiber dimension.

It may or may not be that the local encapsulation and the formation of asbestos bodies prevents that from happening. I don't know to what extent that has been studied.

I mean one purely physical school of thought would say that in fact fairly soon after a fiber of any type enters the body the local reaction is such that it isn't in fact going to penetrate an undamaged cell, and have that sort of effect, so the damage is done quickly.

So as I say, I don't think...you can probably get theoreticians on either side to produce conclusive arguments to show that certain aspects of different fiber types are or aren't critical in relation to carcinogenicity, and I think the truth has to be that people don't know. What evidence there is in animals and cells suggests that the effects are principally physical, but there are long-term chemical differences in biological response and it isn't known how important they are.

DR. DUPRE: Going back to the fiber-type and fiber-dimension schools, and you have, I guess, stated that it is fair to associate you with the fiber-dimension school, just again trying to review my lessons, is it fair to say that as a member of the fiber-dimension school the one possible point that you may wish to concede to the fiber-type school is the association between amphiboles and peritoneal mesothelioma?

THE WITNESS: I'm not sure that...I mean...

DR. DUPRE: Is it again just the dimension?

THE WITNESS: Well, I would suspect that it's just the dimension, particularly because of the extraordinary inconsistency between results on groups that have nominally been exposed to crocidolite, where sometimes there is a substantial

THE WITNESS: (cont'd.) proportion of peritoneal mesothelioma and sometimes there is none. Yes. Yes.

I mean, they could very well be purely physical effects.

DR. DUPRE: Now, can I take it that as a member of the fiber-dimension school you would also associate yourself with the school of thought that I call the industrial-progression-away-from-the-mine school? That is, as you go from mining to milling to manufacturing to using, quite possibly fiber dimension is altered in these progressive stages in such a way that asbestos does become more hazardous the further you move away from the mine?

THE WITNESS: I don't think the last stage of the process is true. I mean, I think up to the final stage of processing I would tend to believe that, although as I say it's very much a matter of unsupported opinion. But I don't...

DR. DUPRE: Now, what do you call the final stage of processing?

THE WITNESS: Well, for example, I mean textile processing, for example, I mean, take the example of the factory where we have studied, where the risks obviously are fairly high. I mean, I don't think that it's necessarily true that the public are at enormous risk in terms of what they do with it.

I mean, if you...I don't think that the form in which it finally goes out to the general public, in general, is likely to produce fibers of the type...in other words, very long, fine fibers. Probably fairly pure ones, so that they don't get stuck and coughed up again, are the type which are likely to be most dangerous. I mean, I think the principal danger does in fact lie in manufacturing and possibly to a lesser extent in initial production and processing.

DR. DUPRE: Can I ask you about this puzzle that I have and it probably shouldn't be a puzzle to me, but it is.

I keep thinking about the U.K. gas mask workers

DR. DUPRE: (cont'd.) study, and as I understand it there were two plants: one made the civilian gas masks with chrysotile asbestos, the other made the armed forces gas masks with crocidolite asbestos. Now I say to myself, insofar as the gas mask manufacturing process is removed from the mines to the same degree in both instances, that might entitle me to assume, if I wanted to be a member of the fiber-dimension school, that the fiber dimensions were the same. Yet as I understand it...

THE WITNESS: Why do you think...I didn't follow... why are the fiber dimensions likely to be the same?

DR. DUPRE: Well, in both instances the asbestos whether it's crocidolite or chrysotile, has been, you know, processed to the same degree, it has had the same kind of post-mine processing.

THE WITNESS: I don't know if that's true. I don't know, but I would think it's unlikely to be true.

DR. DUPRE: I don't know, but in both instances...

THE WITNESS: The things that determine...it isn't only...I mean, it's whether or not a fiber is flexible as well, and the degree to which they break into fibrils and they become finer without becoming shorter, and so on. I don't think that... and I think it's fairly unlikely that even if you put them through the same physical processes you finish up with fibers of the same dimension.

DR. DUPRE: I could then, I suppose, as a member of the fiber-dimension school, say all right, the fiber dimensions were still different and this is what accounts for the larger number of mesotheliomas among the crocidolite gas mask workers.

THE WITNESS: I think there are some data on the actual fiber content of their lungs. I'm just trying to find them in the Lyon proceedings.

I'm sorry. I can't find it straight away.

MR. LASKIN: It may be page 638.

THE WITNESS: Yes.

MR. LASKIN: To 644.

THE WITNESS: Yes. But I'm not sure that I can... I don't want to produce a lightening interpretation and put it on the record if it isn't what the article says.

What I'm looking for is data on the chrysotile gas mask workers and the contents of their lungs.

MR. LASKIN: It's page 647, I think, table seven

DR. DUPRE: Yes. There isn't anybody in that table who is exposed only to chrysotile, though, so it doesn't really resolve the issue.

As I say, the fact that fiber levels as associated with crocidolite processing have never been measured makes it almost impossible to interpret any of these data.

The one critical observation in relation to all this, as I said before, would be...I mean, since crocidolite does remain in the lung and the pleura, a satisfactory analysis of the lungs of people, for example in the Rochdale factory where there is a high incidence of mesothelioma, which is, it has been suggested, due to crocidolite, against people in other factories where there is a lot of crocidolite with a similar incidence of mesothelioma, it seems to me would resolve it definitively.

The question isn't whether there was any crocidolite at Rochdale, but whether the level, the amount in the lungs of people who worked there for about ten years who were known to have a high risk of mesothelioma is of the same order as it is in factories where there was a lot of crocidolite with a high incidence of mesothelioma, and that's a very straightforward study and it seems to me it would produce a definitive answer to this.

DR. DUPRE: One final question along this line

DR. DUPRE: (cont'd.) I have been pursuing.

The Simpson Report, as I understand you, is open to the criticism that the standard recommended for chrysotile is insufficiently stringent, given the new data, correct?

THE WITNESS: It depends what you mean by insufficiently stringent. I think they have underestimated the risk associated with it.

DR. DUPRE: Okay in relation to the risk, insufficiently stringent in relation to the risk associated with it.

THE WITNESS: Yes.

DR. DUPRE: Now, the Simpson Commission, of course, also recommended a differential and tighter standard for the amphiboles, I guess I should say, for amosite. Those standards were, respectively, if I remember rightly, one for chrysotile and zero point five for amosite.

If the Simpson Commission had taken exact account of what you believe to be your correct estimates of the relative risk, would what would have happened have been that the standard they would have recommended would have been point five for both chrysotile and amosite? Or might they have wound up with, you know, point seven five for chrysotile and point five for amosite, or whatever? Could this even be calculated by somebody? Is there a standard that falls out of your relative risk calculations for chrysotile?

THE WITNESS: Yes. I mean, that tab, whatever it is, the Lancet paper...

MR. LASKIN: Tab three.

THE WITNESS: Tab three, where I did the calculations on some detail based on the data that were then available, finished up with the prediction that there would be roughly ten percent of men exposed for fifty years at two fibers might die as a result of the exposure, which would

THE WITNESS: (cont'd.) correspond to roughly five percent exposed for fifty years at one fiber.

Now, curiously, the update of that study suggested that the risks were actually higher than that, as we discussed. But at the same time that estimates of the dust levels were revised upwards, and those two effects happened to cancel out.

So the effect of the later data is to imply that the risk would probably be of the order of five percent, the lifelong risk of death would probably be of the order of five percent after fifty years at one fiber.

As I say, there is a very considerable uncertainty in that. If you ignore the earlier study, your estimate of risk would be higher. I mean, that's after making allowance for the fact that there seems to have been a fortuitously high risk of lung cancer during this later period of the study.

Yes, there's not much I can add to that really. As I say, you've seen the extraordinary range of risk estimates that are produced depending on whether or not you apply the various adjustment factors. That calculation, of course, does not include the adjustment for the assumption that people would only work for four hours a day in the working day at those levels. It's a straightforward calculation based on the revised dust estimates that were provided, and the assumption that the relative risk was between two and three, somewhat higher than was seen in the first part of the study, as a result of the update.

DR. DUPRE: Counsel, any last questions?

MR. LASKIN: I just have a couple of questions.

EXAMINATION BY MR. LASKIN

Q. I just wanted to pursue the Chairman's line of questioning on fiber type for just a moment, in one other area. That's the animal experiment area, and one side of that

Q. (cont'd.) coin seems to be that yes, indeed, you can take any kind of fiber type and you can inject it into rats or have rats inhale it and you will produce incidence rates of mesothelioma which are about the same.

Then the other side of the coin is, I suppose, those witnesses who come forward and say, oh, yes, that it's all really rather artificial because for one thing the chrysotile fibers that are being injected or being inhaled aren't really the chrysotile fibers that you see in the workplace, they are really artifacts that have been specially honed for injection or inhalation, and I guess what I'm struggling for is some notion of what kind of weight does one attach to the animal experimentation?

A. Animal experiments have been done with industrial samples as well as with specially prepared samples. Again, I mean it might take me a few minutes to find them, but they are in some of these...I don't know exactly where...but I know experiments have been done with sampples that were collected in the factory environment.

Q. And reported by whom?

A. I couldn't have a couple of minutes to try and find them, could I? I'm not sure...I mean, I wasn't aware that the experiments had been criticized on those grounds. I think the distinction between real asbestos fibers that come from factories, and artificial ones that are injected into rats, is a slightly false one. I think the only sense in which those experiments...two senses in which they might be irrelevant is that they have the wrong range of fiber size, and that any very long-term chemical effects aren't going to be observed in animals. Those are the two grounds on which animal experiments can be criticized, but I'm not quite sure what a distinction between a real and a synthetic fiber is.

Q. Okay. Just one final area, and it may be my slowness today, but could you...it results from a line of

Q. (cont'd.) discussion you had with Mr. Hardy about Mr. Berry's study at Ferrauto, the friction material study, and you, as I heard you, talked about the subtle statistical problem with that study in terms of analyzing mesotheliomas and fiber types.

Could you just go over that again for me, for my benefit? I know you haven't had a chance to look at the article.

A. I haven't even looked at the latest draft of the paper, but as I understand it the form of the study was that there was one very brief period when crocidolite was used, that in fact it was a factory that used only chrysotile, and there was one very brief period when crocidolite was used, and the basic format of the study was to take cases, mesothelioma cases, and compare them with controls who had been entered into the factory at more or less the same time to see whether there was a difference in terms of the proportion who had been employed in the area of the factory where crocidolite was used.

The difficulty was that in fact, because of the type of work that was involved, these were predominantly production workers who had also had heavy exposure to chrysotile in the past, so it wasn't a fair comparison. I mean, you were basically taking people who already had a heavy exposure and therefore you couldn't, on the fact of it, interpret the results. The analysis was adjusted...there was a significant relationship with crocidolite exposure.

So the situation could be crudely grasped as being that there is a certain distribution among the work force of exposure to chrysotile, let's say, this is sort of the chrysotile concentration you were exposed to and that's sort of, you know, that's five fibers and that's ten fibers and that's fifteen fibers, and so on. That's the sort of pattern of chrysotile exposure. This is fibers per ML.

A. (cont'd.) But in production workers who were also exposed to crocidolite, these people worked in the area that contained crocodilite, and the other group, the non-production workers, if you like, might have had a distribution that looked like this, where that's five fibers.

I mean, where this is their exposure to chrysotile, and these are people who didn't work with crocidolite, I mean, just crudely speaking this is the sort of division that was made, because these people had had heavy chrysotile exposure, the ones who were selectively chosen to work with crocidolite, and so as I say, if you took the crocidolite workers and the no-crocidolite workers, you would find this sort of difference in the distribution of exposure to chrysotile.

Now, it's immediately obvious that if chrysotile is what is causing mesothelioma, then these people have a great deal more exposure to chrysotile than these people, so therefore you are going to find, under either hypothesis, you are going to find that these are the people who get the mesotheliomas.

Now, the only adjustment that was done in the analysis that I saw, and I think it's still true in the version that you've got, is division of the two cohorts into whether or not they were exposed to more or less than five fibers per ML of chrysotile, and this is obviously not a sufficient division because among the people the mesotheliomas are more or less confined to the people exposed to more than five fibers per ML. of chrysotile. It's obvious that if the distributions looked like this, and this is the sort of way that distributions usually look, even among the people exposed to more than five fibers, there is a very much heavier chrysotile exposure in the production workers than there is in the other workers.

So until the data are broken down in this version, it's impossible to say. They may, of course, be impossible to reconstruct, because as in all factories, data on pre-war...this

A. (cont'd.) refers to exposure in the thirties, but this is a very elementary statistical mistake which... statistical difficulty at least...which certainly hasn't been dealt with in the analysis that has been done.

As I said, the easiest way to see to what extent it's true is to look at the other way around, to assume that the chrysotile is the sole cause of mesothelioma and see if you can explain it away in terms of the reported exposure to crocidolite.

But if the results are symmetrical, I mean that would indicate rather strongly at the very least that the study has been misinterpreted and perhaps it points in the other direction.

Is that Okay? I'm not sure if I've made myself clear. But the essence of the analysis was to stratify...the crude analysis showed an association...all the mesotheliomas were in this group...not all, but the majority of them.

It's quite interesting because there were one or two in the other group. I think that should be borne in mind as well, but the majority of them were in this group.

This is highly significant and it remains significant, but very much less so, when the analysis is carried out separately, comparing these people with these people, and these people with these people.

Q. Just briefly, I don't want to get into any long explanation, but how greatly would you adjust the other way? How would you do the analysis in reverse to assume that the mesotheliomas are caused by chrysotile and then adjust for crocidolite?

A. Well, the best way would actually be to try and measure...I mean, they have presumably got some estimates of chrysotile exposure, and you would work out something like the average chrysotile exposure in the case controls and then do the

A. (cont'd.) analysis overall, and then analyze separately for people who worked in the area where the crocidolite was, and people who didn't.

MR. LASKIN: Thanks very much, Mr. Peto.

DR. DUPRE: Mr. Peto, on behalf of all of us, may I thank you most warmly indeed for the two very long days that you have given us, much to our benefit.

Thank you again, sir.

We now rise until such time, either the 11th or whatever of August.

THE INQUIRY ADJOURNED

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THE FOREGOING WAS PREPARED
FROM THE TAPED RECORDINGS
OF THE INQUIRY PROCEEDINGS

Edwina Macht

EDWINA MACHT